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ABSTRACT

This quarterly publication is designed to share information of practical, theoretical, administrative, and general interest. It provides linguistic fieldworkers with news, reviews, announcements, and articles that will stimulate interest in linguistics and help them to stay current with the field. Articles in this volume include the following: "Is Cecil Worth the Bother?"; "Interpreting Cecil Frames: Examples from Chimila"; "An Annotated Bibliography of Basic Acoustic Theory for the Field Linguist"; and "The Amazonian Languages." (KFT)

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Notes on Linguistics, 2000

David Payne, Editor

Volume 3, Numbers 1-4

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FROM THE LINGUISTICS COORDINATOR

Tribute to retiring consultants[1]

The very small print on the inside cover is probably not what most of you concentrate on when you rip open the package containing your eagerly-awaited issue of *Notes on Linguistics (NOLx)*. However, the men and women on this page are some of the oil that makes the machinery of *NOLx* and SIL function well. Our International Linguistics ADVISORS, who are scholars outside of SIL, and our International Linguistics CONSULTANTS, who are SIL members, play a key role in helping keep our linguistic standards high and in helping others in various ways, such as conducting workshops, consulting on papers and publications, and giving advice. If you compare the list of Advisors and Consultants in this issue with the last one, you will note some significant changes. Some people are retiring, some are coming on board. (As a phonologist, I'm tempted to refer to elision and epenthesis here, but I'll resist). In this issue of *NOLx*, I would like to acknowledge the immense contributions the retiring Advisor and Consultants have made, and say 'Thank you for a job well done!' As Isaac Newton commented when complimented on his far-reaching discoveries, 'If I have seen further than others, it is because I have stood on the shoulders of giants.' These are some of the giants who have served faithfully and well. It has been a delight to reflect on their lives and service.

In the next issue, I will introduce the new International Linguistics Consultants. By the way, full references to SIL members' works alluded to here may be found on SIL's web site at http://www.sil.org/acpub/biblio.

INTERNATIONAL LINGUISTICS ADVISOR:

John W.M. Verhaar

Philosopher, linguist, and Jesuit priest—Father John Verhaar has had a versatile career. He is perhaps most widely known as the editor for many years of the journal *Studies in Language*. Verhaar's work has been centered

[1] Thanks for insights and contributions from David Bendor-Samuel, Eugene Casad, Tom Headland, Paulette Hopple, Karl Franklin, Gloria Kendall, Eugene Loos, and Mary Ruth Wise. They of course bear no responsibility for any omissions and defects in my final writing.

in the Pacific area, and he has mainly worked in Japan, Indonesia, and Papua New Guinea. One of his most significant publications was the definitive *Toward a Reference Grammar of Tok Pisin: An Experiment in Corpus Linguistics*, published as a special issue of *Oceanic Linguistics* in 1995. He also worked on a grammar of Indonesian for years before bad eyesight forced him to suspend that project. As shown by these, Verhaar's main interests lay in grammar. He has been a tremendous help to SIL in Papua New Guinea on a number of occasions when he ran linguistics workshops or gave special lectures. As a philosopher, John has also written a basic philosophy textbook, and has had a special interest in philosophical postmodernism. Now living at The Hague, John continues to write in philosophy.

INTERNATIONAL LINGUISTICS CONSULTANTS:

Desmond Derbyshire

In the days when object-initial languages were totally undocumented and thought to be impossible, Desmond Derbyshire spoke up in one of his graduate courses and told the skeptical professor that he SPOKE an object-initial language from Brazil: Hixkaryana. The result was several publications documenting this pattern, such as his 1977 'Word order universals and the existence of OVS languages' in *Linguistic Inquiry*, and his 1979 *Hixkaryana* (the first volume in the *Lingua Descriptive Studies*) and his PhD dissertation from the University of London on *Hixkaryana Syntax*. Des and his wife Grace also translated a New Testament in this language, in the process helping the group regain its vitality as a people, and defending the dignity of Brazilian Indians in an insightful letter in the *Sunday Times* of London. Des is recognized as a leading authority not only on Hixkaryana, but the Carib language family. He is the co-editor (with Geoffrey Pullum) of the *Handbook on Amazonian Languages* (*HAL*) series, with four volumes currently in print. Des is very gifted in both writing and in editorial skills, and he had a tremendous input in helping various SIL members ready their materials for the *HAL*. This was not a new pattern of his life; Des has had a long-standing practice of providing help to many SIL members in preparing publications. Thus it is no surprise that Des headed up the department of Academic Publications for SIL in Dallas for several years.

Des has also been active in training. He has often taught courses at SIL schools, served as a university professor in Brazil for some time, and was SIL's first Training Coordinator. His most recent publication is a chapter on Carib languages in *Languages of the Amazon* (Dixon & Aikhenvald, eds.),

published last year. At present, his goals are completing a dictionary and translation of the Old Testament for the Hixkaryana.

Donald Frantz

Dr. Donald Frantz (PhD 1970, University of Alberta) has concentrated on Algonquian studies for much of his professional career, especially on Cheyenne and Blackfoot (though occasionally taking excursions into other totally unrelated languages such as Chi-Mwi:ni!). Especially from his early studies in Blackfoot, he gained insights on decomposing some lexical entries into separate predications which are only evidenced by variations on possible adverbials and subordinate clauses. His main interest through the years has been grammar, first Transformational Grammar, then Generative Semantics– and then becoming a leading scholar in the field of Relational Grammar. He was actively involved in teaching grammar at SIL-UND for many years, and produced the text which the SIL advanced grammar course at the University of North Dakota used for some time, the 1979 'Grammatical Relations in Universal Grammar'. Alert to the live issues of the day in linguistics, he guided and encouraged publishing by SIL members, and coauthored many articles with them. He was instrumental in helping them write articles for major journals. Don conducted several workshops for the Peru and North American Branches of SIL. One of his contributions to SIL was his development of software that was the precursor of the current WORDSURV, designed to aid in the organization of data collected in language surveys and comparative linguistics. Don is now retired from SIL.

Robert Longacre

Few have had as much impact on discourse studies within SIL as Dr. Robert (Bob) Longacre (PhD 1955, University of Pennsylvania). With his 1977 'A Discourse Manifesto' in *Notes on Linguistics* (still well worth reading), he made a clear case for the need for discourse studies and the inadequacy of only sentence-level grammar. With his publications of *An Anatomy of Speech Notions* in 1976 and the 1999 *Grammar of Discourse*, as well as the many classes of SIL students he has taught, he has alerted generations of SIL students to the need for discourse analysis as a crucial part of language analysis.

Longacre started working on the Trique language of Mexico, which he soon made famous by publishing a paper on its five distinct tone levels, the first of only a handful of languages documented to have that many. He was one of the prime writers to help reconstruct Proto-Otomanguean to a time depth comparable to Proto-Indo-European, and has published on grammar and phonology as well as discourse. Bob has trained many SIL members both in

classes and in workshops on the field on a variety of linguistic topics, and co-authored many papers with others. Not limiting himself to Mexico, he has traveled and written extensively on languages of Colombia, Papua New Guinea, and East and West Africa. In recent years Bob has concentrated on Biblical Hebrew, with works such as his 1989 *Joseph: A story of divine providence: A text theoretical and textlinguistic analysis of Genesis 37 and 39-48*. Bob's passion for the practical comes out in his 1998 'A suggestion for the training of mother-tongue translators' in our sister publication *Notes on Translation*. Bob is Professor Emeritus at the University of Texas at Arlington, and is not yet retiring from academic work, but only from active consulting. He plans to spend the next years concentrating on more Hebrew studies, as well as continuing as the editor of the *Journal of Text and Translation*.

Eunice Pike

Not many people in the linguistics community, inside or outside of SIL, have had firsthand exposure to as many languages as has Eunice Pike. Trained as a nurse, she worked for years among the Huautla de Jiménez Mazatec people of Mexico. 'Big brother' Kenneth was an invaluable help in the early days, but her own linguistic gifts soon became evident as she began to help other people with their linguistic analysis. Her 'Immediate constituents of Mazateco syllables,' co-authored with Kenneth Pike in *IJAL* 13, is still widely cited. Eunice's special gift has been in phonology; she has 42 phonology-related books and journal articles listed in SIL's Bibliography. For years she was one of SIL's stalwarts in teaching phonology in the classroom; her passion was to make phonology accessible to all and to bring her experience to bear on complex phonological systems. One former student still speaks of a phonology handout of hers as 'the best one-page phonology guide' he's ever seen. Her 1992 article 'Analysis of tone systems' in *Notes on Linguistics* 56 is based on her extensive experience with tone systems in Mexico, Papua New Guinea, and Africa. It is still hard to beat for a concise guide to beginning tonal analysis. Outside of the classroom, Eunice has also consulted with many, many other SIL language teams, both in workshops and on an individual basis. The result has been many descriptions and analyses of phonological systems, some of which she has helped with, and many of them co-authored with other SIL members. Eunice has now retired from active linguistic work.

Evelyn Pike

A concern for training students has long marked the career of Evelyn Pike. She has served as the interpreter for her husband Kenneth and made his thoughts accessible to a much wider audience than otherwise would have occurred. This perhaps reached its zenith with the 1977 publication of the tagmemics textbook *Grammatical Analysis*, but a number of other publications testify to her interest in grammar of Mexican languages and practical pedagogy. Evie has been active in SIL classrooms for decades, teaching whatever was necessary, but majoring in grammar. She was also among the first to attempt a controlled experiment in intonation with an infant (her own!), which she duly wrote up and published as the 1949 paper 'Controlled infant intonation' (*Language Learning*, vol. 2).

One of the gifts of both Evelyn and Ken Pike to SIL has been their constant support of younger scholars, both in practical ways such as helping with linguistics manuscripts, and emotional support to continue with their work. Not a few SIL members owe their very presence in SIL today to their backing and personal intervention. Evelyn remains an active promoter of linguistics at SIL's International Linguistic Center in Dallas, organizing 'academic chit-chats' and encouraging linguists to keep pressing on!

David Thomas

Dr. David Thomas (PhD 1967, University of Pennsylvania) has the heart of a consultant and teacher. He also has been one of SIL's most prolific writers, with over 100 items in the SIL Bibliography. Recognized as an authority on Mon-Khmer languages, he has published extensively on these, as well as more pedagogical books such as his 1993 *An Invitation to Grammar* (copublished by Mahidol University and SIL). David is well-known for his excellence in scholarship on his own, but also as a teacher and a consultant to others as well. As a teacher, David has a reputation for making the complex simple. He and his wife Dorothy served for ten years as teachers and thesis advisors at Mahidol University in Thailand, and were recognized for being patient, kind, and helpful there. As a consultant, he has a reputation for wanting to help linguists get started, and has specifically written on consulting as well as on strictly linguistic matters. He has conducted numerous workshops and has a knack for bringing out the best in people.

David's contributions to Mon-Khmer studies were recognized in a Special Volume of *Mon-Khmer Studies* (1996, volume XXVI), which he was instrumental in founding. The tribute found there is a much more complete and adequate one than can be fitted in the pages of this small journal. He continues to do editorial work in Mon-Khmer Studies, and Old Testament

work in Northern Khmer as well as helping out in various ways with SIL's North America Branch.

Mary Ruth Wise

One of the major emphases of Dr. Mary Ruth Wise (PhD 1968, University of Michigan) has been bringing together cross-linguistic and cross-cultural data and patterns into one place. This was the case, for example with her 1978 *Los grupos étnicos de la amazonia peruana,* a major reference on Amazonian ethnic groups of Peru, co-authored with Darcy Ribeiro. Mary Ruth has been a consultant in almost every country in South America where SIL has worked, as well as in the Philippines and other countries in Asia and Africa. One of her emphases and joys has been publishing and editing, helping other SIL members prepare materials for publication. She has been a prolific writer on Amazonian languages, and has made her research readily available to local scholars by writing over half her publications in Spanish. Behind the scenes, however, she has been even more prolific in rendering assistance to other SIL members. She has had a major influence on many dozens of other articles and books which have appeared without her name on the byline, and so has both directly or indirectly been one of the most productive scholars in SIL. Continuing in that role, Mary Ruth has recently become the Senior Editor for SIL. She remains an active conference attendee and writer herself, with her most recent publication being a chapter on small language families of Peru in *Languages of the Amazon* (Dixon & Aikhenvald, eds.), published last year.

—Michael Cahill
International Linguistics Coordinator
■

Is CECIL worth the bother?

Terry Malone
SIL—Colombia

[**Editor's note:** This is the first of a series of three *Notes on Linguistics* articles on CECIL by the author. The second, 'Interpreting CECIL frames: Examples from Chimila', appears in *NOLx* 3.2. The third, an annotated bibliography, will appear in a future *NOLx* issue.]

0. Introduction. Several articles have appeared in *Notes on Linguistics* which encourage use of CECIL[1] as an integral part of field linguistic work (Cahill 1992, Hunt 1994, Baart 1996). While these articles are helpful to field linguists venturing into uncharted CECIL waters, the most effective encouragement to tackle a new and perhaps threatening computer program could be testimony from someone who has found that it has made a significant difference in their own language work.

I have written this article in part to demonstrate the difference that CECIL can make for practical field work, and how it can contribute to significant analytical progress where progress did not seem possible. I also give practical suggestions to supplement those in Cahill 1992 and the CECIL manuals, as well as the more theoretically oriented suggestions in Baart 1996. Lastly, I describe my own CECIL saga to illustrate what the CECIL neophyte might expect out of this program, and what the field linguist must contribute in order to effectively use this tool in phonological analysis.[2]

1. Situation of the language program. My colleague Margrit Hotz and I started working among the Chimila people (Chibchan language family) in Northern Colombia in May of 1986. Due to sociolinguistic factors, including a history of repression and ridicule, Chimilas were very hesitant to speak

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[1] CECIL is the name of a system for acoustic analysis of speech. Originally it was a small, portable, battery-powered system used in conjunction with an MS-DOS personal computer. A current version WinCECIL runs in conjunction with a Windows sound card.

[2] I would like to express appreciation to my colleague Margrit Hotz who has read and commented on two drafts of this paper. She has also provided a considerable amount of logistical support which made it possible to experiment with CECIL, conduct phonological analysis, and communicate the results here and elsewhere. Thanks go to Joan Baart for extensive suggestions, many of which have been incorporated in this version, though he is, of course, not responsible for any deficiencies that remain. Thanks also go to several Chimila speakers who contributed recorded data for analysis. I do not mention their names here due to political conditions in their homeland. The analysis has been conducted under the terms of an agreement with the Colombian government (Ministerio del Interior).

their language in our presence. When we could finally hear some connected speech, we both thought that the language might be tonal.

Although we both were able to do some initial language study with women in exchange for reading and math lessons, the sociolinguistic situation, the living situation of the people which we shared, and their resulting sicknesses (and as a consequence, ours) kept us from being able to hear and learn the language as one might expect.

In May 1989, almost three years to the day after we had begun, we had to flee the area due to plans of subversives to kidnap us. We have been unable to return. When we had gotten over the worst of the ensuing emotional turmoil, we implemented an alternate strategy which involved living in the state capital, helping Chimilas who occasionally travel there, and inviting them to come for workshops or to help us in our study of the language.

The whole situation had made it impossible to check our initial data. Because of the sociolinguistic barriers and mistrust (which was only beginning to thaw when we were forced to leave) neither of us had done much tape recording—just a few texts and phrases on cassette. About the time I had milked that rough-hewed and wooly data to the limit, Chimilas started appearing at the door of our apartment through the intermediary of Colombian friends. But it took time to rebuild enough trust and relationships which would allow us to again work on the language.

2. CECIL joins the team. In the middle of this, I read about CECIL in an issue of SIL's *Notes on Computing* and without hesitation ordered the program, the blue box, and the special microphone. After studying the documentation, and going through the program menu, I by-passed the sample phrases which were included on the CECIL program disk, and immediately started digitizing Chimila phrases.

Looking back on my experimentation and struggles this was the best place to start, both because Chimila was what motivated me, and because I began by loading in entire phrases (I digitized questions, statements, and commands). This gave me an overview of phrasal and sentence intonational patterns, as well as an idea of how the frequency trace drops and rises around certain individual segments. I found that a knowledge of these patterns was useful for later study of stress and pitch patterns on isolated words.

When I started digitizing, I used tape recordings, mostly because no Chimila speakers were accessible to us. I immediately found that the process required some coordination in order to get an entire word, phrase, or sentence on the screen. I also found that it doesn't matter what garbage appears before or

after the utterance, since the program allows one to trim away all extraneous matter. After I had digitized several hundred phrases, sentences, and words, I found that if I wasn't quite sure where the utterance was that I wanted, but was in the right neighborhood, I could start recording, let the tape recorder run until what I wanted to digitize came up, and then hit a key to stop the digitizing. This turned out to be a somewhat delicate matter, because if one lets CECIL digitize for too long of a stretch the version 2.0 program for DOS hangs. So far I have not found this to be a problem in WinCECIL 2.2.

In the beginning I ran CECIL 1.0 and 1.2 under DOS on an 8088 computer (a dual-floppy Toshiba 1200). When I shifted to Windows (on a 386 SXL Compaq) I found that if I ran CECIL 2.0 in a DOS window under Windows 3.1, the program invariably hung whenever I tried to digitize data. This hasn't been too much of a nuisance, because I use CECIL within Windows to double-check data when I want to incorporate data into descriptive work or when I'm doing dictionary work within Shoebox[3]—such occasions are not the optimum time to work on digitizing data.

I quickly found out that if the sentence I was digitizing was longer than the program's 3.33 second default setting, decreasing the sampling frequency allowed me to record longer sentences (up to a point). This eliminates higher frequencies, and results in less accurate sampling, but when one is considering whole sentences one is not usually considering characteristics where this would significantly skew the results. This is not a recommendable procedure if one is specifically focussing on fricative sounds, which involve higher frequencies, but in that case one would probably not be digitizing whole sentences.

The 3.33 second length in the older version easily accommodated the Chimila data I was digitizing. I have had considerably more difficulty matching data and graph length while using WinCECIL 2.2, because it digitizes at 22,050 Hz. This results in a maximum length of 2.98 seconds per utterance, a real nuisance, because I can't get both repetitions of a word or short phrase digitized in one file and up on the screen at once. The only solution I have found is to digitize at 11,025 Hz despite the loss in accuracy.

After digitizing a hundred or so words and phrases, I also found that it was not necessary to insert the phonetic transcription of what I had digitized. I only inserted the transcription when I thought that the word or phrase would make a good illustration for future reference, or when it turned out to be a

[3] Shoebox is a computer database program for field linguistics developed by SIL, primarily for doing interlinear texts and dictionaries.

good CECIL graph for some member of a minimal pair or a perfect illustration of some outstanding phonetic characteristic. Because I transcribed all of my recordings into a data notebook before feeding them into CECIL, and then wrote all measurements that I made after digitizing into the data notebook next to the item in question, I stopped saving every scrap of digitized data. I saved only what I thought merited transcription, or what looked worthy of further study. As time wore on, transcription became easier and more rapid. I often could identify where sounds began and ended simply by examining the shape of the waveform trace.

After some initial fiddling I quit bothering (for the most part) with adjustments to the tone trace. I found that once I made sure the upper and lower limits (in Hertz) on the vertical scale of the data and frequency graphs resulted in a centered frequency and amplitude trace, i.e. with just enough room above and below the curve to clearly show relative changes, other adjustments were rarely necessary. Occasionally it was necessary to adjust the voicing threshold (as in SIL 1992:52ff). If it became necessary to fiddle around too much with this setting, or with the other three settings[4], it meant that the recording was bad. I found that in these cases I was better off re-recording the data.

Once I started building a dictionary in Shoebox, I eventually began to enter a line for the file name of digitized CECIL data under appropriate entries. I have found that notations of CECIL files in the dictionary makes for more efficiency in analyzing phonology and morphology. It is also handy for the design of literacy materials and for planning writers' courses. I have now digitized well over three thousand words and phrases and have saved over 700 of these as CECIL utterance files. This includes experiments with a half dozen or so other languages, a step I highly recommend in order to have a reliable standard of comparison.

My initial experiences led me to use tape recordings whenever I digitize. I have not tried to convince Chimila speakers to talk live into the microphone of the CECIL box or computer microphone. There are several reasons for this. One reason is that the array of cables and apparatus is (to say the least) intimidating to a person unfamiliar with the technology. Another is that we have found that Chimilas have an aversion to saying something in their language exactly the same way twice in succession. Only one woman, after much careful explanation, was willing to try to repeat something the same

[4] The setting 'interpolation gap' no longer exists in WinCECIL 2.2; thus there are only two other settings.

way. And I could almost feel her gritting her teeth as she steeled herself to try to repeat it the same way as she first said it. Even then she couldn't always bring herself to say it the same. Another reason for using tape recordings is that it was already a challenge coordinating the audio input with the computer program—how much more to do so while trying to coordinate all this with another person participating.

I have used two methods of digitizing taped data with the older CECIL box. One is to let the tape recorder play directly into the CECIL microphone, without an intervening cable. CECIL documentation indicates that this is not what the designer of the box intended, but it results in a fairly clean trace on the screen if background noise is eliminated from the working environment. This method obscures slight breaks in the final (F-Smooth) frequency trace useful in identifying some segments. This is not detrimental to the task at hand if one is recording in order to measure segment lengths and uses the waveform and replay feature to position segments.[5]

The preferred method of digitizing data with the older box, and the method which I now use for CECIL 2.2, is to connect the tape recorder directly to the input jack on the CECIL box. If the tape recorder does not have a setting which allows it to output through its speaker and output jack at the same time, one must acquire a jack attachment that allows one to plug in earphones and the line to the CECIL box at the same time. (A stereophonic jack attached to a monophonic tape recorder is undesirable because only one line transmits the signal.) We also use the jack attachment with two lapel mikes for taping data. In this arrangement the taper wears one mike, and the Chimila speaker the other. This went a long way toward reducing self-consciousness on the part of the Chimila language consultant and in general makes for a more even, better quality tape recording. We have taken to taping all data as if it were going to be used with CECIL, whether or not this is actually the case.

This method also works in the Windows version using CECIL 2.2. One connects the tape recorder directly to the input jack on the computer. I was disappointed to find that direct recording (from either a speaker or tape

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[5] I used the first method when I did not have an adequately shielded cable, and was getting an unacceptable level of background interference in my CECIL graphs. The cable method depends most on the quality of the cable. Its success also depends on the internal shielding of the computer, and the tape recorder being used. Interference can even vary according to how the cable and tape recorder are positioned with respect to the computer. I have used combinations of four different computers and three different tape recorders; I have observed differences in interference for every combination.

recording) will not work on my laptop (with a Pentium 166MHz MMX CPU and 32MB RAM)[6], even with all the settings on high. This is an annoyance, because I have sometimes directly recorded segments from my own speech for comparison; in such cases, It is a bother to have to deal with cassette recordings as an intermediate.

3. What CECIL did for us.

3.1 It provided a check on impressionistic phonetic descriptions. Because our main Chimila language consultant was missing a front tooth, I was able to observe some aspects of articulation fairly directly. For instance, [t], [d], [n], [l], and [s] were all obviously dental. There were other sounds where I could see what the tongue was doing, but my descriptions didn't resemble anything I could find in the phonetics textbooks at hand:

- there was an alveopalatal affricate [tʃ] that often sounded like it was preceded by a light dental stop [t̪]—one could see the tongue reaching to the teeth and then sliding back to form the affricate, even when the dental stop was not audible;
- both semiconsonants occurred preceded by lightly articulated stops, a voiced velar stop in the case of the labial semiconsonant ([ᵍw]), and an alveopalatal or palatal in the case of the alveopalatal semiconsonant ([ᵈj]);
- there was a prenasalized alveopalatal semiconsonant in which that alveopalatal or palatal stop intervened between the prenasalization and the semiconsonant ([ⁿᵈj]);
- word-medially there were two alveopalatal nasals: one in syllable onsets that sounded like a normal alveopalatal nasal, and another that sounded like a dental nasal [n̪] in the syllable coda immediately followed by a nasalized alveopalatal semiconsonant or the alveopalatal nasal in the onset of the following syllable;
- the alveolar flap sounded like it was preceded by a glottal stop [ʔɾ];
- in word-initial position prenasalization behaved as expected, but word-medially it sounded more like one would expect from a full nasal stop in the syllable coda followed by a plosive in the following syllable onset;
- a peculiar back high glide occurred in some words between a velar stop and alveolar flap—I perceived it as a high back rounded transition vowel whereas my colleague heard it as rounding on the flap.

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[6] The problem is no doubt due to an inadequate sound card. I should hope direct recording works on newer laptops, but have no experience to know for sure.

While CECIL couldn't tell me directly where Chimila speakers put their tongues when they were articulating these sounds, it often could confirm whether or not the lightly articulated stops actually occurred. I had to come up with a clean enough frequency trace in order to see the break in voicing that indicates the presence of stops. I found that if the final plot wasn't so clean, the raw frequency plot usually gave me the clues I needed. Once my ears became keener, I no longer needed CECIL graphs to confirm the occurrence of these stops. In addition, I learned to watch more closely what Chimilas did with their tongues when they articulated semiconsonants.[7]

I compared CECIL graphs of glottal stops in various positions in the word (including word initially preceding vowels) with graphs of the alveolar flap unambiguously preceded by glottal stop in word medial position and graphs of the flap in intervocalic position with no contiguous glottal stop. These comparisons revealed that the word-initial alveolar flap was not preceded by a glottal stop. I looked into the mouth of my language consultant when she articulated word initial alveolar flaps, and found that I was hearing the turbulence created by her tongue when it flipped against the alveolar ridge.

While CECIL couldn't tell me what phonetic terminology to use to describe the long word medial alveopalatal nasal (fourth bulleted point above), it could tell me whether it was the length of one normal nasal consonant, or two successive ones. In addition, it was not always easy to distinguish between word-medial [ɲ] and [nɲ], but the slow replay and/or length measurements usually revealed which of the two was in question.[8]

CECIL provided raw data which allowed me to confirm my initial descriptions of the behavior of word medial prenasalization, the lightly articulated stop of word-medial semiconsonants in CVCV words, and the behavior of word medial [nɲ] and [ɲw] clusters.

In addition, CECIL data also resolved Margrit's and my disagreement concerning the description of the transitional segment that occurred between some velar stops and the alveolar flap (last bulleted point above). The waveform graphs clearly indicated that a short vowel (40 milliseconds)

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[7] I did not use CECIL frames as sole evidence for the occurrence of these stops. More than one line of evidence was always used to confirm or disconfirm interpretations of what we were hearing. A major line of evidence which I do not usually mention was the reaction of Chimila speakers when we tried pronouncing segments the way we thought they were being articulated. See *NOLx* 3.2, the second article in this series, Figure 1 for an example of [ndj] and [^{d}j].

[8] See *NOLx* 3.2, the second article in this series, Figures 2a-f, 3a-b.

occurred between the stop and the flap. I could not hear rounding of the flap even on slow replay. Nevertheless, when I watched speakers' lips closely, I saw that sometimes the rounding of the transition vowel did extend to the flap, especially in the speech of older speakers. Margrit's first language consultant had been an older speaker, whereas I mostly worked with and taped younger speakers.

3.2 It revealed details our ears didn't perceive. In the process of refining impressionistic phonetic descriptions, CECIL revealed things I probably would have otherwise missed:

- in some words fully articulated nasal consonants occurred in syllable coda position immediately preceding a prenasalized voiced stop in onset position, in contrast with words in which a briefly articulated nasal consonant in coda position was followed by a non-prenasalized voiced stop in onset position;
- labialization following velar stops and nasals, as well as palatalization following dental stops and nasals, had the length of a full segment, and not the subsegmental length that one would expect of a minor articulatory modification;
- The alveopalatal nasal [ɲ], the dental-palatal affricate [tʃ], and the prenasalized alveopalatal semiconsonant [ndj] were each longer than one would expect for single phonetic segments (I confirmed this observation by studying phonetics texts and using CECIL to measure comparable segments in other languages);
- although all voiced stops are prenasalized in Chimila, some cases of word initial voiced velar stop followed by the high back rounded transition vowel and the alveolar flap were not prenasalized, i.e. [g^{u}ɾ] and [^{ŋ}g^{u}ɾ] contrasted with one another in word-initial position;
- high pitch and primary stress did not always occur on the same syllable.

Some CECIL frames clearly revealed the syllable final nasal, with the immediately following prenasalization and the voiced stop. In frames where the boundary between the two was not so clear, one could tell that both elements were there because the total length of the nasal segment was 1.5 to 2.0 times as long as in words in which prenasalization occurred without the preceding nasal consonant. Eventually I noticed that the nasal-consonant-prenasalization sequence usually occurred in disyllabic words, which were in isolation or at the end of a phrase. Hayes 1995 led me to conclude that here Chimila speakers inserted a mora in order to form a minimal bimoraic foot.

In disyllabic words they inserted an additional mora at phrase level in order to indicate phonological phrase boundaries.[9]

In the case of the labialized velar and the alveopalatal segments, comparative evidence, and language-internal distributional and morphological evidence independently suggested that these could be sequences of two segments (/gw/, its stem-medial allophone [ŋw], /nj/, /tj/, and /dj/, respectively). The phonetic data provided further evidence supporting this analysis.[10]

As usual, my colleague Margrit, was the first to observe that some word initial velar stops followed by the transition vowel were not prenasalized, although I was beginning to get suspicious as I carefully worked through checking lists and comparing them. CECIL showed that there was no prenasalization, but it could not tell me how to analyze the phenomena in question. On the basis of distributional and comparative data, and the consistent occurrence of the bilabial semiconsonant with a preceding lightly articulated velar stop, I concluded that the phonetic sequences [kᵘɾ] and [ᵑgᵘɾ] corresponded to phonological sequences /kwr/ and /gwr/, and that [gᵘɾ] corresponded to the sequence /wr/.

The most exciting discovery to me was what CECIL revealed about the relationship of pitch and stress in Chimila. Being a native speaker of English, I had heard high pitch as primary stress. Margrit, whose native language is a Zurich dialect of Swiss German, heard the same. So we had assumed that most words had stress on the first syllable, and words with pitch pattern M(id)H(igh)L(ow) or MMHL had stress on the syllable bearing high pitch. (At this point we did not consider Chimila to be a tonal language.)

But one day, after repeating the word ['kaʰkʷak·ʷa] (pitch = MHL) 'slab of wood' a half-dozen times in an attempt to correct my pronunciation, the language consultant sat back in her chair, looked at me with exasperation, and said, 'Can't you hear the main beat?' I had been putting words of this pitch pattern up on CECIL, but her question confirmed what I had begun to suspect: high pitch was not the main indicator of primary stress, and primary stress was almost always on the first syllable. Although the highest pitch in the phonological word usually was on the leftmost syllable, there were numerous exceptions. In addition, high pitch sometimes was behaving as if it belonged to a phonological subsystem separate from

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[9] See *NOLx* 3.2, the second article in this series, Figures 4a,b,c for an illustration of this phenomenon.

[10] The phonetic evidence by itself would not provide sufficient justification for this analysis.

intonational pitch. I had been pronouncing the offending word as [ka^{h}'k^{w}ak·^{w}a] (MHL) instead of ['ka^{h}k^{w}ak·^{w}a] (MHL) and the woman (who was being much more patient than Chimila culture stipulates in such cases) apparently had sore ears after hearing [ka^{h}'k^{w}ak·^{w}a] (MHL) six times in a row.[11]

In words with pitch pattern MHL the length of the first two vowels was usually the same, and sometimes the vowel bearing the high pitch was even longer than the vowel bearing primary stress. Thus the only reliable acoustic correlate of word-level stress for these words was intensity. I extrapolated from words of this pitch pattern to words with pitch pattern HML, assuming that intensity (all other things being equal) was a dependable acoustic correlate of primary word stress throughout the language.[12] Words of pitch pattern MHL made it clear that I had to separate word-level pitch from intonational pitch. Later it became clear that I also had to separate word-level stress from higher level intonational stress. In fact, in the surface intonational system, intensity, vowel length and pitch interact. It also became clear that the intonational stress system in interaction with the lexical tone system basically behaves as if it were a pitch-accent system.[13]

I concluded (Malone 1998a) that Chimila has an underlying restricted tone system which is mostly obscured on the surface by the intonational and metric systems. CECIL didn't tell me how to analyze it or exactly what it

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[11] See *NOLx* 3.2, the second article in this series, Figures 6a,b,c,d for illustrations of this pattern. Figures 2d,2f,5c,7a,7b illustrate the normal HML pattern in which primary word stress and intonational high pitch (pitch-accent) coincide.

[12] I am still trying to determine acoustic correlates of primary word stress in words with pitch patterns other than MHL. In spite of the objections of a referee, it is patently clear that intensity is the primary accoustic correlate of word stress in words with this pitch pattern, both with monomoraic and bimoraic vowels. In words of other pitch patterns, changes in intensity are often used to mark the pitch-accent nucleus; sometimes intensity functions as and is heard as high pitch. Thus intensity is not as reliable an indicator of primary stress elsewhere in the language, and it was not safe to extrapolate to word patterns other than those with MHL pitch, nor assume that intensity was a reliable acoustic correlate of stress in other word patterns. In most cases I got away with doing this because the locus of primary stress and intonational stress (pitch-accent) usually coincided.

[13] I consider the surface intonational system to function basically as a pitch-accent system for the following reasons: (1) the intonational stress locus shifts, usually on predictable landing sites, but it can also land on extrametrical or unfooted syllables, depending on the intent of the speaker; (2) intonational high pitch overrides lexical low pitch in words of pitch pattern HML where the first syllable is marked for lexical low pitch; (3) only one high pitch is allowed per lexical word at surface level; and (4) many of the same tone rules that operate in pitch-accent languages elsewhere are operating in Chimila.

was, but it told me more was there than met the eye, and it told me what that Chimila woman meant when she said, 'Can't you hear the main beat?'

If we had been able to live among the Chimila people longer, and they had spoken the language more with us, both of us almost certainly would have eventually caught on to what was going on with stress and pitch. In our case, CECIL has made up to some degree for one of the most difficult aspects of our language program: lack of intensive constant contact with the target language. And we found we were dealing with a tonal language after all, albeit a restricted tonal system.

4.3 It helped us with analytical problem-solving. My coworker and I began teaching Chimilas to write their language before we had produced a coherent phonological analysis. As a result we found certain classes of words and segments that were problematic to us and our willing students as we tried to figure out how to write them. Because of the unfortunate longstanding expressions of the Chimila's non-indigenous neighbors to the effect that their language could not be written down, many Chimilas likewise believed that it was not possible to write phenomena in their language that were different from the national language or that speakers of the national language could not articulate properly. It was, thus, a high priority to us to move quickly toward a coherent phonological analysis and ensuing practical alphabet. The problems appeared in the following areas:

- Consonants occurred which were clearly long. Chimila writers effortlessly identified them as double. Other consonants occurred which sometimes sounded long, and which sometimes didn't. Chimilas could not decide whether these were double or single; my coworker and I could not agree either. Pairs began to appear, such as [sakkwi] 'to grind' and [sakkwi] 'to tamp earth by pounding'; Chimilas insisted that these were different, but my coworker and I could not distinguish them.[14]
- We heard long vowels with rising pitch, falling pitch, and level pitch. For pairs such as [ᵐbree?] 'to braid' and [ᵐbree?] 'to lie down' we could not hear a difference, but Chimilas assured us that they were not the same. Usually when Chimilas assured us that a given vowel was double, we would hear pitch sliding up or down.
- In certain positions some long vowels became short; others did not. For some words Chimila speakers could not agree among themselves as to whether a given vowel was single or double. In some contexts neither Chimilas nor I and my coworker could tell whether a given vowel was single or double.

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[14] I write these words and the Chimila words in the next paragraph as we heard them at first.

I kept track of the problem areas, and taped lists of the offending items arranged according to syllable patterns. I also kept lists and taped pairs of words which Chimilas assured us were different, even when we could not identify differences between the two. Then I fed the recordings into the obliging blue CECIL box.

One obvious clue which eventually led to a solution in all three cases, of course, was to measure vowel and consonant lengths in the recorded words. The first step was to establish standards of comparison. In order to do this I measured vowel length in syllables bearing primary stress and in non-stressed syllables in penultimate and ultimate position where the vowels were indisputably single (one mora in length). I found that the average mora length in Chimila was around .14 second (cross-linguistically .1 second is expected in mora-timed languages) in syllables that bore primary stress; vowels were even longer in syllables that bore high pitch.

In determining average vowel lengths in stressed syllables I found it crucial to measure vowels in words that were embedded in phrases, because in isolated words the phrase level intonational (pitch-accent) nucleus usually occurred on the syllable bearing primary stress. If the intonational (pitch-accent) nucleus occurred on a primary stressed vowel its length could extend to .17 second; this approximated the minimum length for canonically long vowels, and accounted for some of the mutual confusion as we tried to help Chimilas write their language.

Once I had established average vowel lengths in embedded words, and had some idea of general intonational processes at phrase level, I could study words in isolation; this involved making the necessary adjustments for phrase-level mora insertion and pitch patterns which are invariably superimposed on isolated words. I usually had my language consultant repeat isolated words twice. This meant that the first word bore higher level phonological patterns typical of phrase pre-peak, and the second showed patterns typical of phrase post-peak. Often this allowed me to separate phrase and word level patterns, especially when I could tape suffixed or other inflected forms of the words in question.

For consonants I began by measuring the length of syllable initial resonants. In most cases these averaged .1 second (one mora) or more. Sibilants averaged a little less than .1 second in length. For syllable initial stops I measured the gap between the first waves indicating onset (release) and the

first waves indicating vowel onset.[15] For word medial single (and lengthened) stops I measured at the point where the previous vowel waves ceased (ignoring the spurious echo which often appears)[16] and the onset of the waves marking the immediately following stop release. The gap for single stops turned out to be about the average length of the mora for vowels. The average length of syllable-final nasal resonants also tended to parallel that for vowels; the average length for semiconsonants (not including the lightly articulated stop) was around .1 second.

Once I had a standard by which I could judge consonant length, I made the first astounding (to us) discovery: word medial consonants in pairs of words such as [sakkwi] 'to grind' and [sakkwi] 'to tamp earth by pounding' were both double; in isolated words analogous to 'to grind' the consonant varied from .2 to .27 seconds, and in isolated words analogous to 'to tamp earth by pounding' the consonant length varied from .27 to .36 second (or even more, depending on the mood of the speaker).

When words such as 'to grind' were suffixed, the double consonant shortened to the length of a single one; when words such as 'to tamp earth by pounding' were suffixed, the double consonant shortened down to a length of .17 to .2 seconds. In the meantime I had been puzzling over the total absence of CVCV patterns for words pronounced in isolation. This distributional irregularity and these length measurements provided all the data needed to come up with an explanation: the two mora double consonants of words such as 'to grind' were actually single consonants in syllable onset position which lengthened in order to form the minimal bimoraic foot which must occur in every Chimila word.[17] Therefore these consonants could be written as single consonants, and the others as double consonants.

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[15] Often the voicing of voiced stops and the onset of vowel voicing overlap. Measurement is somewhat more difficult in these cases. I measure to the point where the influence of the stop on the vowel ceases. This can be detected in the waveform graph, but I double-check its location by using the replay function.

[16] In my data the shape of the vowel waves and the echo waves are quite distinct. The echo is usually audible on replay. It has a tinny, artificial quality which decisively distinguishes it from the preceding vowel.

[17] In other words, an isolated CVCV word would have the footing (μ)<μ >, which is not acceptable in Chimila, because the phonological word must contain at least one trochaic foot. Lengthening results in the acceptable foot structure (μμ)<μ>.See *NOLx* 3.2, the second article in this series, Figures 5a,b,c,d for illustrations of the various consonant lengths.

Why did I not elicit frames, as is standard procedure in such cases? There are several reasons: First, from sentence elicitation and text transcription I knew that it was difficult to frame verbs so as to avoid getting a post-peak phrasal pattern across the whole verb word. If I had tried to frame nouns in subject position, they would have ended up in post-verbal position heavily overlaid with phrase-final intonation and subject-to-phrase level mora insertion. They could sometimes have ended up in sentence-initial position, which was just as bad. Perhaps I could have elicited frames with sentence medial objects. But, there were difficulties with this as well—which brings us to the second reason.

Give the cultural aversion to repeating phrases or sentences exactly the same way twice in succession, it was simply too much to ask of my language consultant. She was already taxing herself just to oblige me at word level. To have insisted on tedious work with frames would have been discourteous, and even worse, a cutural blunder.

Last, it is nearly impossible to convince Chimilas to produce out-of-context sentences which they do not consider to be natural. This was no surprise. Framing often stretches the limits of natural speech, and is an out-of-context procedure which uneducated speakers of a previously unwritten language are not likely to appreciate.[18]

This situation has forced me to tape a lot, tape crucial data more than once, tape a lot of variations of the same lexical root, have taped phrases and text material handy, and be willing to throw out wonderful digitized data when I couldn't get a majority consensus from the samples at hand (usually because I couldn't specify the higher level phonological processes that were operating). Running taped data through CECIL, keeping records of what I had digitized, and listing patterns is slowly making up for the lack of frame elicitation. Basically frames have to be pulled out of the elicited data. In addition I have found myself comparing occurrences of the same lexical roots in differing environments where I already understand what is going on at higher phonological levels; this is something like eliciting a frame in reverse.

When all was said and done the results for long vowels were even more remarkable than those for the long consonants. Long vowels with level pitch

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[18] When I do sentence elicitation (usually as a supplement to text or overheard material) I have taken to setting up contextual situations and asking, 'How would you say X in this situation?' or 'What would you say if...?' Even this procedure does not always produce reliable results.

had average lengths of .17 to .20 second, whereas vowels with sliding pitch had average lengths of .26 to .3 second.[19] Even more remarkable, the shorter double vowel lengths occurred exclusively with vowels with HH pitch patterns, whereas the longer vowel lengths occurred exclusively with vowels with rising and falling pitch patterns. A foray in the phonological literature revealed that speakers of other languages can perceive gliding pitches or tones on vowels that are over .090 second in length (Duanmu 1992:89-90). Therefore it was not safe to assume that pitch on the shorter vowels was level merely because there was not enough time to shift pitch levels.

CECIL had already revealed that pitch could operate independently of stress, and that intensity was the acoustic correlate of word stress in some of these cases. Therefore I used the amplitude trace to determine stress at word level, and consequently on double vowels, which usually occur in positions bearing word-level stress. I have been criticized for doing this. Frankly, it never crossed my mind to depend on allophonic vowel length as a reliable indicator of primary stress, for several reasons: vowels bearing intonational stress (pitch-accent) often approximated the minimal length of shorter bimoraic vowels; the positions of the shorter bimoraic vowels or intonational stress (pitch-accent) sometimes did not coincide with the location of primary stress; and both we and neophyte Chimila writers were confusing monomoraic vowels bearing intonational stress (pitch-accent) with shorter bimoraic vowels. In other words, too many other factors were controlling vowel length.

The same data had shown that there could only be two levels of contrastive pitch, and that absolute pitch varied, depending on position in the word and phrase. Therefore drops and rises in the frequency trace indicated relative pitch. This resulted in a total of six possible combinations of intensity and pitch: HH with highest intensity on the first mora; HH with highest intensity on the second mora; HL with higher intensity on the first mora; HL with higher intensity on the second mora; LH with higher intensity on the first mora; and LH with higher intensity on the second mora.[20]

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[19] Of course these lengths varied, depending on syllable position and number of syllables in the word; I cite averages for words in which phrase-level mora insertion had not occurred.

[20] For examples of the first two patterns, see *NOLx* 3.2, the second article in this series, Figures 7a,b. A referee has suggested that the last four patterns are artificial. Figures 8a,b show near-minimal contrast for the first three patterns. My data includes other minimal and near minimal pairs for these patterns. The last pattern occurs so infrequently and is so often overridden by higher level intonational patterns that it has been impossible to find minimal pairs (but I'm still looking).

Again CECIL left no doubt as to what was going on, once the data had been organized and once the significant variations had been sifted out from the insignificant ones. It revealed other crucial facts: in the vowel shortening environments noted above double vowels with the longer length and rising or falling pitch never shortened to the length of single vowels. Only the shorter double vowels (with pitch pattern HH) shortened to a single vowel.

Eventually I came up with an analysis which satisfactorily explained the CECIL data and the anomalies, plus accounted for most of the problems that we and the fledgling writers were experiencing. In underlying structure Chimila has monosyllabic bimoraic vowels, i.e. the double vowels with shorter length and level pitch, and disyllabic bimoraic vowels, i.e. the longer double vowels with rising or falling pitch. [ᵐbree?] 'to braid' turned out to contain a monosyllabic bimoraic vowel ([ᵐbre·?]), and [ᵐbree?] 'to lie down' contained a disyllabic bimoraic vowel ([ᵐbre.e?]).

There was a problem with this explanation: according to current phonological literature monosyllabic and disyllabic long vowels should not co-occur in the same language. I had even found a flat-out statement by a leading phonologist that this could not happen (McCarthy 1986:218-219)[21]. On the other hand, Hagberg 1990 claimed that what I was proposing for Chimila occurred in Mayo, an Uto-Aztecan language spoken in Mexico. Hagberg made a good case for his claim, based on stress behavior alone. For Chimila, I had even more evidence supporing my proposed analysis on the basis of CECIL alone.

Not wanting to rely completely on CECIL evidence (given that I am not a trained acoustic phonetician, plus have to work under conditions in which I can't control speaker input, elicit data frames facilitating reliable statistic analysis, nor access unlimited numbers of speakers), I looked for other evidence as well. I found that the two classes of bimoraic vowels were clearly distinguished by distributional restrictions (monosyllabic bimoraic vowels occur preceding /s/, /nj/, and /tj/, as well as nasal consonants and semivowels in syllable codas, whereas disyllabic bimoraic vowels do not) and morphological restrictions (transitive verb stems of the form CVV are

[21] He does mention possible exceptions in a footnote, and alludes to them in the text of his paper.

alwsays monosyllabic whereas intransitive CVV stems are always disyllabic, and reduplicative processes are distinct for each class of bimoraic vowel).[22]

As it turned out, vowel shortening also distinguished between the two classes of long vowels: monosyllabic bimoraic vowels shortened, whereas disyllabic ones did not. Vowel shortening occurred for speakers who had a low tolerance of unfooted syllables;[23] usually these were younger speakers. Monosyllablic bimoraic vowels shortened and disyllabic bimoraic vowels did not. The lack of LL patterns was accounted for when independent data, comparative evidence and distributional irregularities finally led me to realize that syllables bearing lexical low pitch (i.e. low tone) either terminated in glottal stop or a glottal off-glide [ʰ] and that bimoraic disyllabic vowels had originated from underlying CVCV sequences in which the consonants were identical. The rightward shift of higher intensity in three of the six pitch-intensity patterns could be explained if on the basis of an independent intensity shift I assumed that patterns with shifted intensity were a way of marking lexical high pitch (high tone) on bimoraic vowels which would distinguish it from phrase level intonational stress (pitch-accent). (Malone 1998b clarify further, and presents detailed evidence for the two classes of bimoraic vowels; pending revision, a more recent version of this paper will be published by The Southwest Journal of Linguistics in 2001).

Again the reader will notice that CECIL provided me with crucial data, but it could not tell me how to analyze or interpret what I had found. Nevertheless, without CECIL, this language team and neophyte Chimila writers would likely still be floundering along. Now that I know what is going on, I tape problematic words in various morphological configurations and feed the results to CECIL. The analysis allows me to figure out their underlying phonological structure based on the CECIL graphs. I have also been able to construct lists of words and minimal pair sets which serve to teach potential authors how to express the crucial phonological distinctions using a provisional alphabet.

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[22] At the 1998 LSA meetings, where I presented a paper on this analysis (Malone 1998b), a linguist studying Iroquoian languages told me of a similar claim in Seneca.

[23] In the theory of Hayes 1995 (and subsequently in Optimality Theory) not all syllables in a word have to be included in phonological feet. For instance, in the word siʰnaka 'night' the footing is as follows: (μμ)μ‹ μ›. The first syllable is closed by a glottal off-glide and forms a complete bimoraic trochee, the second syllable is unfooted (there is no way of including it in a foot), and the third is extrametrical (by rule it cannot be included in foot construction).

5.0 What CECIL hasn't done for us. In the course of data manipulation and analysis it is as important to pay attention to what does not work as to what does. Here I will describe several things I tried to do with CECIL as I studied and digitized Chimila data, but which did not produce the desired results, either because of the nature of the language being studied, or limitations of the CECIL program.

5.1 Spectrographic analysis. I could not resist playing with the spectrograph function of CECIL, but found quickly that it was of limited help in the study of Chimila phonology. I experimented in the following areas:

- I made and compared spectrograms of the segments [tʃ], [^{d}j], [ndj], [ɲ], [nɲ], [n:] and [n]. Although spectrograms distinguished quite nicely between all of these segments, I found that it was easier and less time-consuming to use a combination of the waveform graph, frequency trace (raw and smooth), slow replay and my ear to distinguish between these segments when their identity was in doubt.
- I experimented to see if there was a relationship between the quality of the preceding vowel and length of the following stop, as occurred in two related languages. I was looking for a shortcut to the tedium of measuring stop lengths on the waveform graphs. I found no correlation whatsoever.
- I used spectrograms to make sure that vowel raising for /a/ and vowel lowering for /e/, /u/ and /o/ were occurring where I thought I was hearing it (i.e. word-finally). I was not entirely sure because of my tendency as a native speaker of English toward reduced vowels in precisely those contexts. Once I established that this was indeed the case, I abandoned spectrograms.

For work on a different language, I would not hesitate to experiment again with spectrograms, simply because the value of the spectrograph as a heuristic device seems to depend to some degree on the nature of the phonology being studied.

5.2 Overlay graphs. When I first started working on pitch and stress, I used the overlay function quite a bit. I would overlay the smooth frequency trace and the amplitude trace. Once I had established that pitch and stress were operating independently of each other, I rarely had to overlay the two. I could determine what I needed to know if I put both traces up on the screen side by side. I expect to use the overlay functions again in the future when I study the intonational system in more detail. I also hope to use overlay

frames as illustrations to summarize my analysis of lower level pitch and stress in the phonological description.[24]

5.3 Percentage calculations. In his discussion of the interpretation of frequency graphs, Geoffrey Hunt emphasizes the technique of measuring frequencies and calculating percentage drops through time (Hunt 1995, chapters 9 and 10). When I first began to use CECIL, I applied this technique to my digitized data. I found that it was not particularly useful in determining whether or not lexical pitch (tone) existed in Chimila. I later realized that this was due to the heavy overlay of a vibrant intonational system on top of a restricted tonal system.

The technique is however (as implied in Hunt's writings) necessary in languages where downdrift or downstep are in operation. I suspect that it would be also of use in studying intonation in contour tone languages such as Vietnamese or Chinese. I expect to use this technique when I study in more detail the pitch-accent system of Chimila, (the surface result of the intonational system overlaid on the restricted tonal system) and its relationship to discourse grammar.

In spite of the limited value of this technique in the analysis of a language like Chimila, I have found that it provides a non-impressionistic method for describing intonation contours for questions, commands, exclamations and statements. A non-impressionistic description would make it possible to compare these contours cross-linguistically in a revealing manner. This is not currently possible, given the impressionistic descriptions typical of the majority of phonological descriptions published to date.

5.4 Nasality and glottal off-glide [^{h}]. When I started digitizing data, I had great hopes that CECIL would provide data that would unambiguously identify vowel nasality, and the glottal off-glide [^{h}] (acoustically equivalent to a voiceless vowel). Furthermore, I .hoped that it would decisively discriminate between a heavier version of the glottal off-glide [^{h}] which native speakers could hear, and a lighter version which they didn't hear. After running spectrograms and digitizing a fair amount of suggestive data, I gave up using CECIL as a discriminatory tool for both nasality and the glottal off-glide.

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[24] This will necessitate going back to the CECIL box and CECIL 2.0, because CECIL 2.2 does not overlay the amplitude and frequency traces on the screen, nor in printouts. I have not yet experimented with SIL's Speech Analyzer 1.03 or 1.06a (intended as a replacement for the CECIL programs) to see if it makes up for this lack.

I gave up on nasality chiefly because I thought that it did not function underlyingly in the lexicon. I later found that it did, but that it manifested itself in segmental form at the surface. One of the manifestations turned out to be the heavy glottal off-glide that speakers had no problem hearing and insisted on writing. The other is syllable-final nasal consonants. At the time I was experimenting with nasality I did not have access to literature which would have told me what to look for in spectrograms. More recently I found Maeda 1993, who describes the effects of nasality on French vowels and discusses the interaction of nasal and oral vowel formants.[25]

After some experimentation, I then digitized hundreds of recordings. As a result I found I could identify nasality and the heavier glottal off-glide by close inspection of waveform graphs in CECIL. Both could be identified by slight but definite changes in intensity and amplitude of the vowel section of the waveform graph. The raw frequency plot usually showed traces of the heavier glottal off-glide. Surface phonetically nasalized vowels did occur in Chimila preceding or following nasal consonants. CECIL could not detect them where they followed nasal consonants, but because phonetic nasalization is (predictably) heavier preceding syllable final nasal consonants, it was able to detect it on those vowels. Not all data graphs showed the nasality; it only appeared in good quality recordings.

6.0 Conclusions. Baart 1996 states that CECIL appears to be underused as a field linguistics tool. My experience is in accord with this observation, though a couple of good descriptions of its successful use are Edmondson et al. 1992 and Mortensen 1994. Hopefully this paper makes it clear that if one is moderately computer literate, one can learn to take advantage of what CECIL has to offer with little or no specialized training. The basic principles of data manipulation and phonological analysis have not changed just because CECIL can now be in the field linguist's tool box.

At the same time this paper should make it clear that in order to effectively use CECIL one must learn to efficiently manipulate data. Alder and Roessler (1977:1-2) have the following to say regarding that topic (emphasis is mine):

> COLLECTION OF DATA is the process of obtaining measurements or counts. Valid conclusions can result only from properly collected or from representative data... ORGANIZATION OF DATA is the task of presenting the collected measurements in a

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[25] Ladefoged and Maddieson 1996.298-300 have a brief section on vowel nasalization but I did not find it as useful as Maeda 1993. Figures 2d,2e,7a in *NOLx* 3.2, the second article in this series, illustrate the effects of allophonic nasality on waveform graphs. See Figures 4a and 6a for illustrations of the heavier version of the glottal off-glide [ˀ].

> form suitable for deriving logical conclusions... ANALYSIS OF DATA is the process of extracting from the given measurements or counts relevant information, from which a summarized and comprehensible numerical description can be formulated... INTERPRETATION OF DATA is the task of drawing conclusions from the analysis of the data and usually involves the formulation of predictions concerning a large collection of objects from information available for a small collection of similar objects.

This is an apt description of how CECIL should be used. The program does not collect the data—that is still the responsibility of the field linguist. The program cannot tell the linguist what data to collect—the linguist must also make that decision. It is perhaps the most important decision that one must make as one uses CECIL.

Concerning data collection Moroney (1951:120) comments:

> It is a common pastime in many organizations to collect vast quantities of data... with the vague intention of submitting them to analysis 'one day when things aren't so busy'.... Data should be collected with a clear purpose in mind. Not only a clear purpose, but a clear idea as to the precise way in which they will be analysed so as to yield the desired information.

The field linguist must also organize the collected language data into a form which CECIL can use—the program cannot do that either.

The program does measure, and then extracts RELEVANT INFORMATION, presenting it to the linguist in the form of various kinds of graphs. Again the linguist has to decide whether the graphs contain usable information. The interpretive process is entirely the responsibility of the program user, as should be evident from this paper, the documentation, and other articles on CECIL previously referred to here. One cannot expect CECIL to come up with the background knowledge nor provide the necessary intuition which tells the linguist what is useful out of the analyzed and collected data.

I mention these points, because I have had to interact with field linguists who expect CECIL and other computer tools to do things they cannot do, or who are intimidated by the technology. These interactions have inspired me to write this series of articles. My hope is that more field linguists will consider using this technology as a supplemental source for evidence during the course of phonetic description and phonological analysis.

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REVIEWS

Introducing phonetics and phonology. By MIKE DAVENPORT and S. J. HANNAHS. 1998. Oxford: Oxford University Press. 196 pp. Cloth $70.00, paper $19.95

Reviewed by FREDDY BOSWELL
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Earlier this year, while undertaking a PhD qualifying course in phonology, I came across this volume by Davenport and Hannahs, professors of Linguistics at the University of Durham. They state in the preface that 'this textbook is intended for the absolute beginner who has no previous knowledge of either linguistics in general or phonetics or phonology in particular'. While that is indeed their purpose, their approach does not 'talk down' to someone who, like me, has studied linguistics formally, and been engaged in field work of an Austronesian language for more than ten years. This volume is an excellent encapsulation of the study of human speech and sounds.

The beginning chapters examine the basics of articulatory and acoustic phonetics, presenting such topics as physical production of sounds, supra-segmental structure, and an overview of the production and classification of consonants and vowels. Also, there is a good chapter on acoustic phonetics. The second half of the book deals with basic principles of phonology, including features, phonemic analysis, phonological alternations, processes and rules, and phonological structure, including autosegmental phonology. The last chapter is a treatise on the tension between abstract and concrete relationships in phonology, and touches on issues of learnability, synchrony and diachrony, and plausibility.

While no particular theoretical framework is promoted, the general model which is used is generative phonology. The primary source of data is English, and this is helpful for many readers who will be tackling the concepts of speech and sounds by using a known rather than an unknown language.

The authors have a succinct and clear writing style. The examples and exercises they have chosen enable the student to quickly grasp what they are teaching. I recommend this book both for the absolute beginner, as the authors intended, and also for the experienced field worker who wants a reference book for recalling key concepts.

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Semantic analysis: A practical introduction. (Oxford textbooks in linguistics). By CLIFF GODDARD. Oxford: Oxford University Press. 1998. 411 pp. Paper $24.95

Reviewed by ROBERT D. BUGENHAGEN
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Goddard's textbook lives up to its title. It is a truly PRACTICAL introduction to doing semantic analysis utilizing the technique of reductive paraphrase in a Natural Semantic Metalanguage (NSM). This is a theoretical approach pioneered by the Andre Borgusławski and Anna Wierzbicka in the sixties and seventies and since developed by Wierzbicka and a number of other colleagues. For those unfamiliar with the approach, its distinctives are:

- attempting to construct precise definitions/explications of the meanings of a wide range of lexical items and grammatical constructions
- using NSM, which has a tightly constrained syntax and very small lexicon of semantically simple lexical items (or semantic primitives) which should, theoretically, have ready equivalents in any language of the world. The current set consists of about sixty items and includes concepts such as: I, YOU, SOMEONE, PEOPLE/PERSON, SOMETHING/THING, TIME/WHEN, PLACE/WHERE, THIS, GOOD, BAD, KNOW, FEEL, SAY, DO, WANT, LIVE, DIE, BIG, SMALL, CAN.
- with the goal of producing a definition (or definitions, if there are multiple senses) that explain(s) the full range of collocations which the lexical item or construction is observed to exhibit.

This book is an introductory textbook. Each chapter is provided with exercises and discussion questions, and solutions for some of the exercises are included at the end of the book.

As two examples of definitions using NSM, consider the following, reproduced from pages 94-95:

X feels *sad*=
Sometimes a person thinks something like this:
I know something bad happened.
if I could, I would do something.
I know I can't do anything.
because of this, this person feels something bad.
X feels like this

X feels *unhappy*=
Sometimes a person thinks something like this:
Something bad happened to me.
I don't want this.
if I could, I would do something.
because of this, this person feels something bad.
X feels like this

Defining emotion terms in this way relies on the use of a prototypical scenario which typically triggers the said emotion, a description of the kind of feeling produced 'feels something (very) good/bad)' and then, optionally, further envisioned responses. For example, *angry* would have a component along the lines of 'I want to do something bad to someone', and an evaluative component like 'I/People think this is good/bad'. From these two samples, it will be noted that the definitions are formulated using simple or kernel sentences and a very basic vocabulary. Therefore, they should be relatively easy to translate into any language. Meaning components which the two lexical items share are ideally formulated identically.

Given such definitions, it is easy to see exactly how the two items are postulated to differ in meaning. In this case, *unhappy* has an obligatory personal orientation 'something bad happened TO ME' which *sad* lacks, and *sad* has a component of resignation or helplessness 'I know I can't do anything' which *unhappy* lacks.

This approach has been applied in typologically, genetically, and geographically diverse languages to a wide range of semantic fields including: speech-act verbs, emotion terms, body-part terminology, aspect, modality, evidentiality, causation, artifacts, animals, and key cultural traits. The EMPIRICAL, lexicographic emphasis on actually attempting to DEFINE words from a wide range of semantic fields in radically different types of languages distinguishes this particular methodology from many other approaches to semantics, which most other approaches have tended to confine themselves to a small number of semantic fields and focused mainly on major European languages.

The book begins with three chapters that introduce the reader to some of the phenomena typically studied by semanticists such as: generic-specific relationships between meanings (i.e. hyponymy); incompatibility of meanings (i.e. different types of incompatibility); similarity of meanings (i.e. synonymy); entailment; contradiction; and paraphrase. The first three chapters are introductory in nature. The student of semantics is introduced to the nature of meaning and traditional and contemporary theoretical approaches to semantics. The introduction also discusses a number of problematic issues in semantic theory such as:

1) the culture-specific and anthropocentric nature of the meanings of most lexical items;
2) the distinction between meaning and reference (the act of referring to things in the 'real' world);
3) the distinction between encyclopedic/scientific knowledge of the referents of a lexical item versus those aspects of its meaning which all speakers of the language would know;
4) the boundary between semantics and pragmatics;
5) discreteness versus 'fuzziness' in meaning; and
6) whether or not there are semantic universals.

Ch. 2 has a particularly good, brief section on common faults in definitions.

G criticizes a number of other linguistic approaches to meaning such as truth-conditional semantics, cognitive linguistics, and structuralist/ componential accounts. He notes the following sorts of failings in these approaches:

1) restriction of their analyses to only a very small number of semantic fields;
2) use of ad-hoc, specialized technical terms, semantic components, and formal devices which seem unlikely to be part of the universal mental endowment of human beings;
3) analyses which fail to account for the full range of collocations which a lexical item exhibits; and
4) semantic analyses of lexical items which are far more abstract and obscure than the actual items themselves. This last criticism assumes the maxim that definitions of words should always be framed in semantically simpler terms than the words being defined.

After the introductory chapters, the remainder of the book consists of a series of case studies in which the NSM approach is applied to items from the following semantic fields: emotion, color, speech-act verbs, discourse particles, motion, artifacts and animals, causatives, and grammatical categories. The defined items come from a wide range of languages: English,

Russian, Polish, Spanish, Malay, Japanese, Australian aboriginal languages, Ewe, and many others. In the concluding chapter, G discusses some of the newer developments in the NSM approach:

1) newly proposed candidates for semantic primitives;

2) constraining the syntax of the definitions;

3) the evidence from child language acquisition for semantic primitives; and

4) explicating key cultural traits/characteristic ways of behaving.

One of the most common tasks in linguistic field research is seeking to understand the meaning of a new lexical item. The NSM approach is an eminently practical way of investigating and explaining meaning cross-culturally. Based on the current reviewer's experience it has the added benefit that native language teachers with limited education seem to readily take to using the methodology.

The present volume would make a valuable addition to any field linguist's library. It is well-written and could be of real benefit in helping a researcher to capture the meaning of difficult-to-define items.

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Reported speech: Forms and functions of the verb. Theo A. J. M. Janssen and Wim van der Wurff, eds. 1996. Amsterdam: John Benjamins Publishing Company. viii+311 pp. Cloth $79.00

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Field linguists immersed in a 'minority' language somewhere far from Europe and North America, familiar with the linguistic literature on that language and its near relatives, need to know first of all that this is a very focused book—apart from a brief final paper on Chinese, another comparing English and Bengali, and occasional passing references to works on other languages, this book is about Slavic, Romance and Germanic languages. Accounts of inventories of four formally distinct degrees of (in)directness of speech (as in Aaron 1992) or of functions of reported speech unfamiliar to speakers of Indo-European languages (as in Larson 1978) are not to be found

here. For a broad coverage of reported speech in languages of diverse stocks, try Coulmas 1986.

The above is a description, not a criticism. The 1993 Leiden colloquium on which the volume is based did have some 'cross-linguistic' aims, though almost entirely within Indo-European. But its greater emphasis was on 'the intra-systemic approach to the study of speech', seeking to understand reported speech in language more completely by investigating 'possible connections between its properties and those of other forms and constructions'—in this case, verbs (2).

The book is thus focused in a second way, in its concentration on verb-related phenomena such as TMA systems. For example, to report in English indirect speech what some people said about their condition at the time of their speaking, I would usually use a past tense in the embedded clause *They said they WERE sick*. But the corresponding sentence in Russian would use a present tense in the embedded clause, back-translated roughly as *They said that they ARE sick*. The papers in this volume do not merely report such phenomena in the languages considered, but go on to explore what else there is about languages like English on the one hand, and Russian on the other, that turns out to be correlated with this difference in tense use in indirect speech. In the process, light is shed on pronouns, other deictics, and various pragmatic notions.

As sometimes happens with collections of papers from a colloquium, most of the papers presuppose a fair bit of familiarity with a lot of the notions and terms referred to. For those of us not up on current literature on reported speech, especially in Indo-European languages, that often makes for heavy going. Yet there are still some ideas worth digging out and thinking about how they could help with questions of form and function of reported speech in some other, unrelated language.

For example, Theo A. J. M. Janssen's 'Tense in reported speech and its frame of reference' (237-259) explains 'the relationship between tenses in the combination of reporting and reported speech' [cf. the above English and Russian examples—GLH] 'as a referential relationship which is not time-based' (237). Specifically, it turns out that which tense one uses in the embedded clause (*... that they were sick*), given a particular tense in the embedding clause (*They said...*) depends on the 'frame of reference for the situation of an embedded clause'. This frame of reference is not always to be determined just by hearing the embedding clause—it 'can be established by the embedding clause, by a situation in the text preceding the embedding clause, or by a situation inferred from world knowledge' (256). Put that way, it may sound obvious—interpretation of parts of utterances can depend on other parts of the utterances, earlier utterances ('co-text'), or information

not mentioned but just assumed as common knowledge to speaker and addressee. Yet the study does help focus our attention on the interplay of syntax and pragmatics and suggests questions to ask ourselves about tense in whatever language we're investigating.

Another example: A. Machtelt Bolkestein's 'Reported speech in Latin' (121-140) includes a section comparing the form of clauses (complements) embedded under verbs of speaking (indirect speech) with those embedded under verbs of mental activity (BELIEVE, UNDERSTAND...), verbs of perception (SEE, HEAR...) and verbs of emotion (BE GLAD, REGRET...). His brief demonstration that the syntax of the embedded clauses is not the same for each of these classes is, again, not revolutionary. But I found it a salutary reminder of the need to look at these various semantically-defined classes in some detail when describing the structure of complement clauses in any language; and his framework (with subcategories within those mentioned above) provides a useful starting-point.

Finally, in Abraham P. ten Cate's 'Modality of verb forms in German reported speech' (189-211), I found the distinction between typical roles of modal elements and of deictics worth thinking about with regard to other languages: he says with regard to the former that 'one of their dominant features is their continued orientation towards the reported speaker', unlike the latter, 'which tend strongly to a reporter-orientation in reported speech' (208).

I wouldn't give this book high priority for every field project's individual library; but a library in an administrative center could add it to their collection as a moderately useful source of ideas for grappling in detail on what's going on with tense, mood, aspect and other systems in reported speech in any language.

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Apes, language and the human mind. by SUE SAVAGE-RUMBAUGH, STUART G. SHANKER and TALBOT J. TAYLOR. 1998. Oxford: Oxford University Press. 244pp. $29.95

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This is a two-part, four-chapter volume written as a defense of the notion that the bonobo ape (*Pan paniscus*) has capability in language acquisition such that it should force us to reconsider the nature of communication and its relation to cognition based not just on human but also non-human language acquisition. It affirms the possibility that forms of communication among higher (non-human) mammals exists, but present day language research is unable to account for it because the theoretical basis from which language acquisition research operates disallows such recognition. I approached Savage-Rumbaugh's book as both biologist and field linguist.

Ch. 1 is by Savage-Rumbaugh (professor of Biology and Psychology at Georgia State University) who gives a summary of her study of language acquisition of Kanzi, a male bonobo ape (closely related to the chimpanzee). S-R says that since the bonobo vocal tract differs markedly from the human one in terms of its angle (from mouth to throat), position of the larynx, attachment of the tongue, etc. it is physically incapable of producing sounds used in human language (even though it tries), yet its ear and brain are able to perceive and decifer human speech, at least to some degree. S-R claims they can learn to understand spoken English, even rapid speech, in a sentential context. They can learn written symbols that correspond to many of the spoken words, although their hands lack the motor control skills of human hands--they are quadripeds and their callused hands are used primarily for walking and maneuvering and not for detail work like writing. For this reason S-R devised a chart with a large number of lexigrams (simple designs, numbers and single words), each indicating different notions of human communication (nouns, verbs, adjectives, etc.) to be used by the bonobo to communicate with the human researcher. (The bonobo would point to the idea/s he wanted to communicate.) Kanzi learned to read the printed symbols and used those symbols to communicate with people. S-R describes at length a number of her sessions with Kanzi demonstrating Kanzi's language learning capabilities. For example, he learned the names of different sites within the study area at Georgia State University's Language Research Center which includes an adjacent section of forest. He learned by name what was located at each site and indicated such with the lexigrams. He then communicated what he wanted, at which site the desired object was

located, and which pathway he wanted to take to get there, and when. During games, Kanzi could also indicate a desire to be chased or tickled, indicating who was to be the agent or patient. He could communicate evaluative ideas such as 'good' and 'bad', and use them in appropriate ways. He communicated multiple-word ideas frequently. Also, Kanzi could communicate imaginary information as if it were real, and respond appropriately to verbal requests, even if/then sentences. He could communicate about happenings of the past. He could follow the thread of a conversation, even if he wasn't a participant in the conversation.

The problem with S-R's claim is that the language acquisition processes cannot be observed through dialogue (assuming that speech is necessary for evaluating how an ape understands). Therefore, when S-R presents evidence asked for by her critics, doubts are immediately cast over her work, or the bonobo's abilities. For this reason Chs. 2 and 3 present an alternate theoretical and philosophical base for evaluating S-R's work. S.G. Shanker (Ch. 2), of York University approaches the study as a philosopher and psychologist, and T. J. Taylor (Ch. 3) of William and Mary College approaches the study as a linguist. Both contend that contemporary philosophical positions are inadequate for evaluating her work and force us toward the conclusion that 'apes only appear to understand. They don't really.' Or, 'S-R must be interpreting more into the ape's capability than is there.' In this realm Shanker discusses the philosophical base of Descartes and the Cartesian view of language and language acquisition and how that view has left us incapable of evaluating ape language skills because they see apes as discontinuities in nature. To people of that persuasion supposed 'animal intelligence' is invariably explained away. They argue that an individual is the only one who knows whether his behavior is based on reason and whether or not it results from a conscious effort. But humans are the only ones capable of discussing their own behavior with another person. And the tools they use to evaluate cognition in people do not work in evaluating the same idea in non-humans. Taylor's chapter says that no progress will be made in evaluating ape language cognition until there is agreement in the scientific community on common methods for doing so.

The final chapter is written mostly by S-R. Through it we see the philosophical base on which all the rest of the book rests: that we should study the bonobo monkey because, from an evolutionary point of view, our human roots began with them (morphologically, physiologically and linguistically). Using Bruner, Quine, Chomsky, and Taylor's discussions of language acquisition in humans, she suggests, based on her work, that apes have the possibility of both reason and consciousness like humans do, but we

are unable to recognize the processes because apes cannot express themselves by means of oral communication. The biologist has to 'break the language barrier', so-to-speak, which requires not just a linguistic study but one of animal behavior as well. S-R recognizes the difficulty involved in accepting her evidence, but says the scientific world will have to rely on the reports of scientists who do this kind of work, just as it relies on an anthropologist's findings from the study of some primitive group of people. The only other alternative for the scientist would be to redo her work, or evaluate all of her video work on the bonobo.

The experiments sounded reasonable, well thought out, and well documented. The book's purpose was to persuade through logic that the bonobo has much of the same perceptive capability and the same structure for syntax as humans. The book was of a defensive nature. Some described experiments were not detailed, and some conclusions had other possible interpretations (though she often referred to her conclusions as the 'obvious ones', and continually used 'fact' and 'certainty', for situations that were clearly theoretical in nature).

Human cognition is evaluated on the basis of verbal communication skills. To S-R, not enough attention is given to how behavior interacts with learning of those verbal skills, and with the development of social skills that are related to it.

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Language and the brain. by LORAINE K. OBLER and KRIS GJERLOW. 1999. Cambridge: Cambridge University Press. 206 pp. Hardback $54.95. Paperback $17.95

Reviewed by CHERYL JENSEN
SIL—Brazil

The focus of research of neurolinguistics is the breakdown of language due to problems in the brain. Through identifying the part of the brain involved, more and more is being learned about how the brain is organized for language. Studies focus on people born with brain defects which affect language, on people who have suffered loss of certain language skills through trauma or tumors in specific areas of the brain, and on people whose language skills have suffered through a more generalized atrophy (i.e. dementias of the types frequent in elderly people). Bilinguals, with or without neurological problems, are also the focus of neurolinguistic research.

Separate chapters in this book deal with the classification of aphasias, factors underlying the syndromes, specific types of aphasias (childhood aphasia, right brain damage, dementia), disorders that affect reading, and bilingualism. Each chapter includes a summary of what this particular area of study tells us about language.

Neurolinguistic studies show that there are many separate but interconnected parts of the brain that serve specific functions in relation to language. Separate areas of the brain deal with the phonological, syntactic, and semantic components of language. Damage to the part of the brain dealing with phonology can result in phonetic distortion, sometimes even giving the impression of someone speaking with an accent. Damage to the part of the brain having to do with syntax can result in the inability to use functors or inflectional affixes. Damage to the connections between parts of the brain results in speech that is fluent but incoherent. Violations of Grice's three conversational maxims are illustrated in demented patients.

Most language functions, in particular the analytical aspects of language, are handled in the left side of the brain, while pragmatic features of language are handled by the right side of the brain (in most people). For example, intonation is handled on the right side of the brain. However, it has been shown that tone is handled on the left side of the brain, as it has an analytical rather than pragmatic function.

O&G assume that basic word order is determined by grammatical parameters, and that variation in word order is used for pragmatic reasons, as is the case in most languages. They give an example of someone who was unable to vary the word order (or to comprehend word order variations) because of damage to the part of the brain which dealt with pragmatic aspects of speech. This must be a language-specific effect. Since the word order of the Brazilian language I study is determined much more by pragmatic features than by grammatical, it is questionable to what degree the notion of 'basic word order (based on grammatical relations)' in this language has any psychological reality.

Although the organization of our brains is often compared to computers, studies have shown that there is more redundancy built into our brains than we program into computers. For example, there are two parts of the brain which are involved in reading. A portion of the left hemisphere deals with orthographic (i.e. phonemic) skills and a portion of the right hemisphere deals with logographic (i.e. whole-word) skills. As readers of English, we use both portions of our brain, since we use both sight word and decoding skills. Lesions in one of these two areas will make reading more difficult for

us, but not impossible. The more analytic an orthography is, the heavier the demand on the portion of the left hemisphere. By contrast, logographic writing systems (e.g. Chinese) depend primarily on the portion in the right hemisphere of the brain. Among Japanese, who use the 'kanji' (logographic) system for most substantives (lexical morphemes) in combination with the 'kana' (syllabic) system for functors and for borrowed foreign words, there have been cases reported where alexics have trouble with one of the two systems but not the other.

One area of interest in studying bilingualism is code switching: Where in a sentence will a speaker switch from one language to another, and what does this tell us about the psychological reality of grammatical rules? Another area is language recovery in bilingual aphasics: Which language is recovered first and why? As field linguists working with minority languages, we are in an especially good position to study code-switching in bilinguals or multilinguals and check out the conclusions proposed by neurolinguists.

The future of neurolinguistics lies in four different areas: neurophysiology of language, the relationship between language and cognition, brain imaging, and cross-linguistic studies. It is in the area of cross-linguistic studies that field linguists have the potential for making contributions. The types of language breakdown that take place in isolating languages like Chinese are different from those in highly synthetic languages. Given the typological diversity of languages that we study, descriptions of language breakdown would contribute towards determining what are, in fact, universal principles of language organization. It can be tricky to extract the necessary data, since aphasics often develop ways of masking their language problems. However, techniques have been developed to extract such data (by H. Goodglass and students). A careful linguistic description of such phenomena should require a good linguistic understanding of the language itself, as a point of departure for describing aberrations.

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Texas Linguistic Forum 40: Working papers in phonetics and phonology. By AMANDA R. DORAN, TIVOLI MAJORS and NISHA MERCHANT GOSS, eds. 1998. Austin: Department of Linguistics of the University of Texas at Austin. 132 pp. Paper $9.00

Reviewed by MIKE MAXWELL
SIL—International Linguistics Department and Academic Computing

Introduction. The working papers in this volume fall into two categories: phonology studies (using Optimality Theory, or 'OT'), and acoustic phonetics studies. As is typical of working papers, they are a bit rougher than papers which would be published in a journal. There are typos (most self-correcting), and the style is a bit repetitive at times. The collection will be primarily of interest to those linguists wanting to read a couple short papers in Optimality Theory, to get a flavor of that approach; or to those interested in the use of acoustic phonetics, and particularly in statistical analysis of that data.

Phonology papers. Nisha Merchant Goss 'Interaction of the coda condition and prosodic word structure in Malayalam compounds' looks at 'the interaction between the coda condition and the prosodic word structure of two types of nominal compounds in Malayalam' (a Dravidian language). Sub-compounds consist of a modifier-head structure, and act prosodically as a single word (cf. *blackboard* in English); co-compounds behave as a head-head structure, with each stem of the compound being a separate prosodic word. Word-final vowel epenthesis, nasal assimilation, and gemination behave differently in the two types of compounds. The original analysis, due to Mohanan (1986), was based on the theory of Lexical Phonology, and relied on strata (rule blocks) to distinguish the behaviors of the two kinds of compounds. Goss instead attributes the differences to the differing prosodic structures of the two sorts of compounds. This study thus illustrates how a derivational account may be re-cast in a theory (OT) which does not allow recourse to (rule) ordering.

Claude E. Mauk 'An OT analysis of Sanskrit syllable structure' uses OT to analyze syllable structure in Sanskrit. Needless to say, none of the data is new, and the analysis does not break new theoretical ground. But it may be useful for the reader wondering how syllabification works in OT.

Adrianne Cheek 'An OT analysis of Southern Paiute "Final Features"' looks at three classes of morphemes in Paiute: those that spirantize a following morpheme-initial consonant, those that geminate such a consonant, and those that nasalize the following consonant. Cheek attributes the different

behaviors to a morpheme-final segment, a segment which is invisible when word-final. A similar derivational analysis, but in derivational (rather than declarative) terms, is given by Chomsky and Halle (1968:344-351). Cheek claims as an advantage of her analysis that the constraints she invokes are cross-linguistically common, rather than language-specific.

Phonetics papers. All the papers in this section combine instrumental phonetics with statistical analysis. It is clear that these two technologies must be applied hand-in-hand, at least when looking at the level of phonetic detail represented here. I recommend the study of these papers (or similar ones) to anyone trying to apply instrumental techniques to other than the simplest problems.

Tivoli Majors 'The effects of stress on vowel-to-vowel coarticulation in English' is an instrumental study of coarticulation effects between vowels in successive syllables. Most previous studies looked at schwa. Majors' study, on the other hand, focuses on the vowels /i/ and /o/, paying particular attention to differences attributable to stressed and unstressed variants. The importance of this is that English schwa represents a peculiar vowel in articulatory terms. Whereas most English vowels have a clear target—a location in the vowel space which the speaker appears to be trying to achieve—it is not clear that schwa has any such target. Two references were missing from Majors' paper, Browman & Goldstein, and Cohen & Cohen. She kindly supplied them (see References).

Amanda R. Doran 'Boundary tones, downdrift, and pitch range in Elomwe' is an instrumental study of intonation in a Bantu language of Malawi and Mozambique, with grammatical (rather than lexical) tone. The study compares downdrift and pitch range for several sentence types, including statements, WH-questions, yes-no questions, and 'incredulous questions' ('The woman spoke with the man?!'). In a recent review in *Notes on Linguistics*, Joan Baart (1999) comments that few SIL members have published anything on intonation. This article could serve as an example of what such studies could accomplish.

Jun Da 'Syllable duration in Mandarin tone sandhi and the Auditory Enhancement Theory' examines some of the acoustic cues besides F0 that influence identification of tones in Mandarin. These cues, such as length or the timing of the turning point in a contour tone, have been argued to enhance (make more perceptible) the tone contrasts. While the study is to some extent inconclusive, it points out something to look for in other tone languages.

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Dynamic conceptual semantics: A logico-philosophical investigation into concept formation and understanding. By RENATE BARTSCH. CSLI Publications. Cambridge: Cambridge University Press. 1998. 296 pp. Hardback $64.95. Paperback $24.95.

Reviewed by STEVE NICOLLE
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Concepts are frequently invoked in cognitively-based theories of language use, but fundamental questions such as what concepts are, how they are formed, and how they affect and are affected by utterance interpretation are often left vague or not discussed at all. Bartsch's dynamic conceptual semantics attempts to answer these questions. Her analysis, although formalized and philosophical in approach, resembling situation semantics and DRT (discourse representation theory) in many respects (although differences are discussed in Ch. 5), yields a number of points of agreement with cognitive theories of communication like Relevance Theory. After summarizing the general content of the book, I will discuss Bartsch's account of metaphor, since this is one of the occasions on which Bartsch interacts most closely with situations of actual language use.

Bartsch begins (Chs. 1 and 2) by discussing and formalizing concept formation. Concepts are defined as 'representations of properties of things and situations' (8), where a property is a regularity in the world (33). Concepts are formed through the 'structuring of sets of data by ordering relationships based on judgements of similarity (identity) and difference (especially opposition or contrast) under perspectives.... Concept formation

with respect to a certain expression is completed if and only if the structures are stabilized' (33). This stabilization is the result in part of increasing internal coherence among structured data sets, and also of social pressure in the form of 'observed, approved of, and corrected linguistic usage' (7). This means that concepts are not innate. Bartsch therefore rejects a Fodorian language of thought and hence mentalistic characterizations of concepts. Concepts are not entities in the mind, rather,

> we can only recall memories of situations, or imagine situations, or refer to situations that belong to the concept. What is psychologically real then is that we can imagine and present examples for a concept; we can form sequences of examples and by this create something like a schema of concept realizations. This is all there is to the psychological reality of concepts. (47)

Bartsch views understanding and concept formation as two aspects of an on-going process. Unproblematic understanding strengthens the stability of concepts and problematic understanding leads to the creation of new concepts. Ch. 3 discusses how we understand sentences, focusing on quantification, negation and modality, and is largely an adaptaion of DRT to conceptual semantics. Ch. 4 discusses correction in concept formation, including concept narrowing and broadening, and marks a shift of emphasis from formalism to philosophical discussion. In Ch. 5, propositional attitudes are discussed, focusing on belief and desire as supposedly the two basic attitudes on which all other propositional attitudes are predicated. Finally, Bartsch notes some similarities between her theory and connectionist models of neuro-psychological processes (discussed in Ch. 6), with a summary in Ch. 7.

Section 2.2 (106-130) contains an account of creative metaphors and metonymies. Bastsch's basic point, which she shares with Relevance Theory, is that there is no difference in principle between the interpretation of metaphoric and so-called literal utterances—both are context-dependent processes of concept formation. A 'literal meaning' is simply the most commonly used or conventionally prior sense. Bartsch argues that, in interpreting a metaphor, a subpart of the properties of the metaphoric term are combined with properties of the situation, under a certain perspective (for example, 'color', 'form', behavior', 'natural kind' and 'artifact'). The fact that some of these properties may not be expressed in a given language, or that more than one property of the metaphoric term may be intended, explains why metaphors are felt to be creative and why they can be so difficult to paraphrase and translate. Creative metaphorical use generates polysemic conceptual complexes, rather then merely making use of existing conceptual complexes (where a conceptual complex is defined as a complex of concepts which are expressed by a given expression under different

context types). This corresponds with, for example, the Relevance Theory claim that creative (or 'poetic') metaphors achieve relevance by generating a number of 'weak' implicatures. What Bartsch's account of metaphors adds to an account such as that of Relevance Theory, apart from rigorous formalization, is integration into a more comprehensive account of concept formation in general than other theories of semantics and pragmatics provide.

Linguists with primarily descriptive interests will probably find this book too philosophical to be of immediate practical use. The high level of formalism, especially in the first three chapters, will also deter many readers. Also, the editing is poor; at least one reference is not in the bibliography and there are occasional problems of English style and grammar, in part because the author is a non-native speaker but also because of the complexity and length (up to 68 words) of many of the sentences. However, there are a number of points of contact with actual language usage (such as the discussion of cross-linguistic differences and polysemy in the term *rice* on pages 70-71). The rigour of Bartsch's approach has a lot to commend itself to anyone interested in basing a cognitive theory of language use on an explicit and psychologically plausible account of concepts and concept formation.

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A prosodic model of sign language phonology. By DIANE BRENTARI. 1998. Cambridge, MA: Massachusetts Institute of Technology. 376pp. $45.00

Reviewed by STEPHEN PARKHURST
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For approximately 40 years linguists have been looking into the phonology of signed languages.[1] Just as spoken words can be broken down into a variety of features that can be used to analyze the phonological processes in a language, so too signs can be divided into features.

[1] In 1960, William Stokoe published the first-ever linguistic analysis of a signed language. Stokoe chose to refer to the basic units of a sign as '*cheremes*' from the Greek word *cheir*, for hand. Today most linguists prefer to use 'phonology' since the phonological processes used in spoken and signed languages are basically the same.

The crux of the argument behind Diane Brentari's model is that signs can be divided into two types of features: inherent features and prosodic features. **Inherent features** are those that are relatively static and co-occurring, such as underlying handshape and place of articulation. As seen in the illustration in Figure 1 of the sign for SHOE in a Madrid dialect of Spanish Sign Language (LSE), the hand must have a shape (at the beginning of the sign two fingers are extended together) and a location (fingertips touching the tip of the nose). Though linguists separate these things, they all occur at the same time. Another feature, orientation, fits in the category of inherent features. The hand must be facing a specific direction and therefore co-occurs with the handshape and location. In the sign SHOE the palm is facing the signer and the tips of the fingers are pointing upward. However in this model the orientation is not directly represented in the formalism; rather, it is implied through the relationship between the place of articulation and the specified part of the hand.

Figure 1: SHOE in LSE

A second set of features is **prosodic**—those that change in relation to time. These are movements, including changes in handshape, location, and orientation. In the LSE sign for SHOE illustrated in Figure 1, there is a change in location and a change in handshape; the orientation stays basically the same. The trick for the theorist is how to represent both inherent and prosodic features in one unified representation.

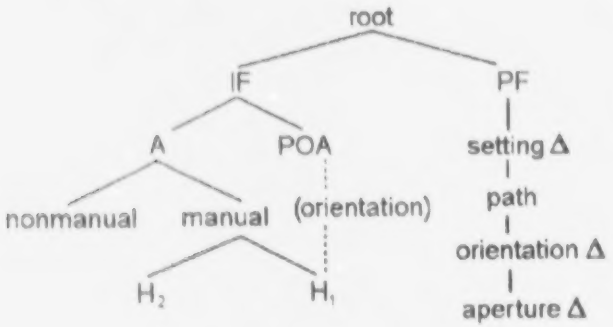

Figure 2: Basic feature tree in the Prosodic Model

Brentari suggests a branching feature tree, shown in its most basic form in Figure 2. The root branches into inherent features (IF), consisting of articulators (A) and place of articulation (POA); and prosodic features (PF), consisting of changes in setting, path, orientation and aperture.

She goes on to assign timing units (x-slots) to the PF branch. Brentari uses a two-slot distinction that corresponds roughly to beginning and end, unlike previous models that define a three-slot syllable (hold-movement-hold (Liddell and Johnson, 1983) or location-movement-location (The Hand Tier Model, Sandler, 1986)). The feature tree for the LSE sign SHOE is presented in Figure 3.

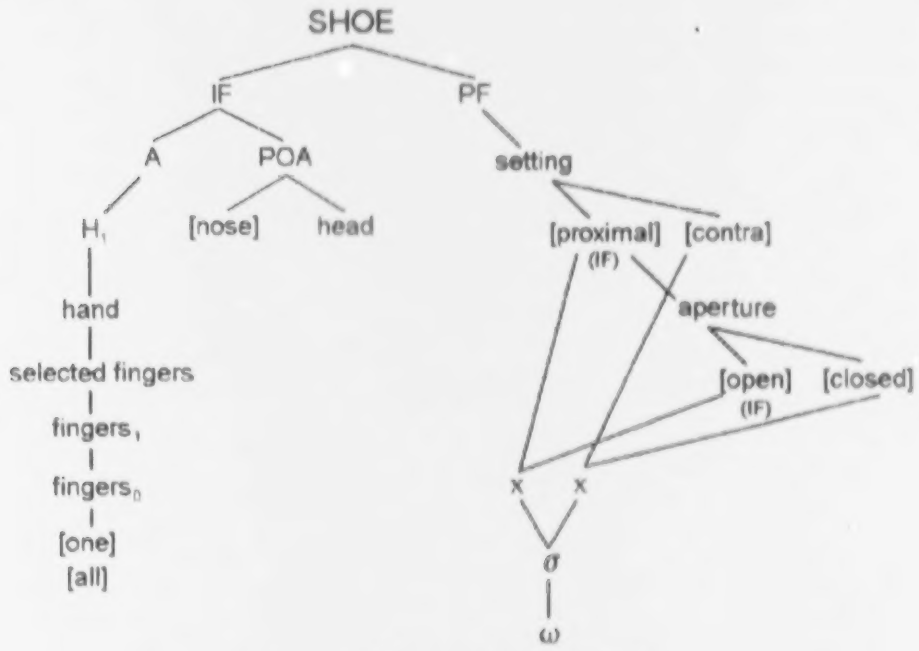

Figure 3: Feature tree for SHOE

In the latter chapters of the book Brentari shows how this combination of inherent features, prosodic features, and a two-slot syllable can account for a number of phonological processes such as lexicalized fingerspelled forms, reduplication in noun-verb pairs, and a variety of issues involving symmetry and asymmetry in two-handed signs.

One of the strengths of Brentari's model is that it draws on the strong points of six different models of sign language phonology and numerous models used in the analysis of spoken languages. She gives a good summary of the leading phonological theories and points out the similarities and differences

between each model and her own. She gives ample reasoning for each of her decisions as to the shape of her feature tree and shows convincingly that her system works with a wide variety of phonological issues.

Brentari uses an Optimality Theory (OT) framework to demonstrate the phonological processes. However, as the name of the book specifies, this is a prosodic model, not an OT model. Those who are not familiar with the formalisms of OT will not be hindered in understanding prosodic theory. In fact, the first *tableau* (an OT formalism) does not appear until page 269. On the other hand, a more thorough utilization of OT constraints would likely shed some light on some areas (the configuration of the non-dominant hand, for example) that she states are not as concise as they might be.

This book is helpful in giving an overview of the different ways linguists are currently analyzing sign language. Many of the phonological issues that Brentari addresses using examples from American Sign Language (ASL) apply to LSE as well. Some of the previous models such as the Hold-Move model are helpful in analyzing syllable structure but less insightful on phonological issues such as the symmetry and asymmetry of two-handed signs. By incorporating both inherent and prosodic features in one unified model, it is possible to analyze phonological processes that involve static features such as handshapes, orientations, and locations, as well as processes that specify changes in these features over time. In order to really understand these two types of processes, it is important to see how they interact with each other, something that is only possible in a unified model.

There are sections of the book that are very detailed and at times hard to follow. But the level of detail is necessary in order to adequately depict the complexity of signed languages. This detail more easily allows the model to be applied to any sign language but may discourage those who are looking for a quick answer to a phonological problem. Nevertheless, I recommend that any sign language linguist interested in phonology make the effort to understand this model, as future developments in the field of sign language phonology will undoubtedly find their roots here.

For linguists who are not directly involved in sign language work, this book may prove interesting but not likely to be very helpful in solving phonological problems in spoken languages. However, those who are specifically interested in issues involving prosodies, feature geometry, co-articulations, and syllable structure in spoken languages might find some useful application of this model. Perhaps the difference in modality (hands vs. tongue and visual vs. aural) may provide significant new insights into these topics.

As the linguistic community has been discovering for more than 40 years, the more we learn about how signed languages work, the better we understand the wonderful mystery of language.

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Essays on time-based linguistic analysis. By CHARLES-JAMES N. BAILEY. 1996. Oxford: Clarendon Press. Hardcover, 423 pp. Cloth $99.00

Reviewed by UWE SEIBERT
SIL—Nigeria

Bailey has been proposing a 'paradigm shift' in the study of language for the past thirty years. This book is a collection of his essays on the topic, often using strong and provocative terms in rejecting earlier models and in defending his own theoretical outlook. The book consists of an introduction written by Peter Mühlhäusler, ten essays by Bailey, a glossary, a large reference section and an index.

In his introduction, Mühlhäusler explains the purpose of this book and the initial reason for preparing it (1):

> ... to make available to a wider audience a representative selection of C.-J. Bailey's views on theoretical linguistics. The initial reason for such a volume was a practical one. It occurred to me whilst I lectured in Bailey's department at the Technical University of Berlin between 1976 and 1979. The students I helped prepare for their final State exams often complained about the difficulties of finding certain of Bailey's writings and the absence of an easy introduction to them.

B explains in a prologue how the essays (which have undergone some editing) were selected (42):

> The writings have ... been selected with as much regard to current difficulty in obtaining copies as to questions of size, balance, importance, and the like—though the collection does try to represent some of the most important general aspects of developmentalist analysis.

Instead of going into the details of the different essays, I will summarize the basic assumptions of the developmentalist framework and the main points of B's criticism of the so-called 'static-reist framework' that is concerned with a single invariant variety of language:

- The static-reist paradigm fails to represent the reality of language which 'is processual and relational, the relations being generally fixed even as the items (structures, sounds, formations) are in flux, varying from style to style and (in given styles) from speaker to speaker where speakers differ in class background and regional usage' (23).
- Time should not be excluded from linguistic description but be built right into it: 'time or (to avoid confusions with clock time) temporality is a fundamental and integral part of analysis' (23). And, 'developments in time account for variation—whether among styles, age groups, classes, or regions' (25).
- Developments in time result in what is called 'implicational patterning': 'What is more marked is later than and implicates what is more marked' (58).
- It is necessary to differentiate between 'connatural' and 'abnatural' developments. Connatural developments are bioneurolinguistically caused and can be predicted, abnatural developments (e.g. language contact and planning) 'are by their very nature much less predictable' (25).
- Language change takes place more frequently in contact than in isolation: 'diversity of place does not cause languages to become different unless they come in contact with other languages in those different places' (294).
- The study of pidgin and creole languages can shed light on possible directions of change: 'language development will never be adequately understood until we are clear about how languages come into being, and ... we should use observable knowledge from the present to infer possible developments of the past.' (291)
- Synchronic-idiolectal analysis of languages is not a theory, but an '*yroëth*' ('theory' spelled backwards), 'something claiming to be a theory which may have a notation and terminology but fails to achieve any deep-level explanation (i.e. beyond the lower level of 'It is no

> accident that ...' —which is, of course, also important) or any predictions of new subsystems' (378).

B's rejection of the static-reist paradigm is reflected in the choice of language, which can be very polemical and at times ironical. Here are just a few examples:

> That synchronic idiolectal analysis is the only basis for any linguistic analysis worth doing is as unargued and unexamined as absurd; it is retained despite its inadequate and counterintuitive nature, which is so out of harmony with current scientific thinking (32).
>
> ...minilectalism remains a superstition—what I have called the SAUSSURIAN SUPERSTITION—in so far as superstitions lack defensible, intuitively satisfying bases (39).
>
> One can hardly escape the conclusion that phonemes, morphemes, and isoglossic dialects are to linguistics what phlogiston, caloric and aether were to the physics of an earlier epoch (88).
>
> Static-minilectal grammarians can be likened to colour-blind individuals who cannot understand the joys of colour (p.100).

The glossary (369-78) presents and explains a number of terms which were either newly coined by the author, are used in a special way, or else are of special interest within the developmentalist framework. This glossary will be useful by anyone 'Trying to talk in the new paradigm' (Bailey 1971).

The reference section (379-404) lists all the articles and books written or edited by B (more than eighty), starting from 1968 until the publication of the book under review. One also finds in the reference section a variety of publications of other linguists, some of which (e.g. Mayerthaler 1981, Mühlhäusler 1985) are recommended for better understanding the developmental framework.

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Bislama: An introduction to the national language of Vanuatu. By DARRELL T. TRYON. Canberra, Australia. Pacific Linguistics, Series D, No. 72., Department of Linguistics, Research School of Pacific Studies, the Australian National University. 261 pp.

Reviewed by JIM STAHL.
SIL– Vanuatu

Darrell Tryon is an authority on the languages of Vanuatu, including the national language, Bislama. This book is written as a language learning guide for expatriates working in Vanuatu who want to learn the national language. Accompanying his book is a set of cassette tapes covering the 16 lessons. One advantage of having a set of these tapes is being able to hear a well-spoken Bislama that has little borrowing or code-switching. For many expatriates learning Bislama in Port Vila, the capital, they tend to hear Bislama mixed with English or French.

Each language learning lesson in the book is structured with texts and comprehension questions, dialogues, vocabulary lists, grammatical explanations and exercises and drills. The texts are stories told by Ni-Vanuatu (Vanuatu people), taken from real-life situations. The various topics of the texts give the language learner an understanding of cultural ways and the history of Vanuatu. The dialogues represent daily life situations in Vanuatu, which help the language learner use Bislama in different domains.

The first section of the book is a short description of the development of Bislama as a pidgin, creole and language. T summarizes his research of Bislama's origins, from the early days of whaling in the South Pacific. Comparing the language examples given in this textbook with what is heard spoken today in Vanuatu, it is obvious that Bislama is continuing to change.

In the next section, T writes about the sound system of Bislama. This is a challenge as there are several varieties of Bislama spoken in Vanuatu, reflecting the numerous languages of Vanuatu. The Bislama spoken on Tanna Island, for instance is different from the Bislama spoken on Epi Island. There are lexical as well as sound differences, and idioms that have no meaning outside the one island. These varieties of Bislama are mostly influenced by the lexicon, grammar and sound system of the speaker's first language. T notes other variations in Bislama throughout this book. In a country that has over 100 languages for a population of less than 200,000 people, Bislama varieties can vary considerably.

The language learning lessons tend to focus on grammatical analysis and explanation. This is helpful to linguists. It is questionable how understandable these grammatical explanations would be to non-linguist language learners.

While I was beginning to learn Bislama in the early 1990s, I used this book as a reference. After having spoken Bislama on a daily basis for eight years, processing this book a second time was a good review. It made me aware of grammatical patterns I had neglected to learn, or had acquired without being aware of them.

Bislama has changed noticeably in both spoken and written forms since the research was done for this book. For instance, I do not see current exclamations like *kas* and terms of address like *brat* 'brother' in the texts and dialogues.

T refers the Bislama language learner to an out-of-date dictionary. There is now a well-documented Bislama dictionary in its second edition, Crowley 1995, which would be more suitable.

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Projections and interface conditions: Essays on modularity.
By ANNA-MARIA DI SCIULLO, ed. 1997. Oxford: Oxford University Press.
257 pp. Cloth $60.00

Reviewed by JOOST ZWARTS
SIL—Kenya

This book is a collection of papers that came out of two research projects at UQAM (Université du Québec à Montréal) about MODULARITY and INTERFACES, two very general concepts in generative grammar to which probably most of the current research done in that framework can be related. Modularity is a way of explaining linguistic facts from the interaction of independent 'modules' in grammar, such as Case theory and Theta theory. The idea of interfaces is roughly that properties of grammar should ideally be explained from the constraints that meaning and phonetics impose on

grammatical form. The papers in the volume approach morphological and syntactic issues from the angles of modularity and interfaces in the spirit of the Minimalist Program of Chomsky (1993). Given the place of origin of the papers is it not surprising that most of the linguistic data come from French and English. Except for those interested in specific grammatical or theoretical issues, the book seems to be of limited interest to field linguists.

Anna-Maria Di Sciullo in 'On word-structure and conditions' argues that words may be full X-bar structures during the derivation, but should end up as X^0s (heads) by head-adjunction to form the right interface with performance systems. In this way she tries to explain several structural and semantic properties of morphologically complex words.

In his paper 'On come syntactic properties of word-structure and modular grammars' Paul Law discusses which grammatical properties are shared by morphological and phrasal structures, concluding that both kind of structures are generated by the same component, but that Theta theory and Control theory do not apply within words.

In another contribution by Anna-Maria Di Sciullo, 'Prefixed-verbs and adjunct identification', the prefixes *re-*, *de-*, *a-* and *en-* in French are distinguished in terms of their structural position in the word structure and their feature composition.

Elizabeth Klipple argues in her paper 'Prepositions and variation' that prepositions universally involve three conceptual categories that can be mapped to syntax in different ways, which allows for a characterization of the division of labor between verbs, prepositions and other categories in different languages.

The Visibility Hypothesis that says that noun phrases are case-marked because they have to be 'visible' for Theta-marking (Chomsky 1981) is attacked by Mireille Tremblay in her paper 'On the modularity of Case Theory: A case against the Visibility Hypothesis'. She shows that, since predicative noun phrases and adjectives in languages such as Latin are inflected for case, this hypothesis has to give way to a more general characterization of the distribution of case.

In 'Argument projection, thematic configurational, and case theory', Jeffrey S. Gruber and Chris Collins syntactically decompose verbs like *knock* into abstract conjunctions of two or more VPs. As a result, English turns out to be a serial-verb language underlyingly, that differs from West-African languages like Igbo, Ewe, and Yoruba in the way the verbs are 'spelt-out' at the interface with phonetics.

This idea that thematic relations should be determined by syntactic structure is further developed by Gruber in a paper called 'Modularity in a Configurational Theta Theory'. He analyzes the thematic structure of a wide variety of English verbs by means of enormous syntactic trees put on paper in an unusual notation.

The contribution of Johan Rooryck is a paper called 'On passive as partitive quantification' in which he tries to explain certain (semantic) restrictions on passive by assuming that the subject in a passive has a quantificational part-whole relation with the participial predicate.

In the last paper of the volume, 'On the syntax and semantics of local anaphors in French and English' Pierre Pica and William Snyder argue that local anaphors like *himself* are not identical in reference to their antecedent but refer to a physical or psychological aspect of their antecedent.

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■

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FROM THE LINGUISTICS COORDINATOR

Introducing SIL's new International Linguistics Consultants

In the last issue, we bade farewell to our retiring International Linguistics Consultants, a truly remarkable group of people. In this issue, I introduce our new International Consultants, who are eminently qualified to follow in the footsteps of their predecessors. You will be hearing from them in the years to come!

Rodolfo Barlaan

Rudy Barlaan (Ph.D. 1986, University of Texas at Arlington) has worked as a linguistics consultant for many years in the Philippines. He assisted on the translation of the Isnag New Testament. Rudy's main interests are in grammar and discourse, but he has a broad range of expertise in other areas as well, as shown by his roles not only as a linguistics consultant in the Philippines, but also as a translation consultant and language-learning consultant there.

Cheryl Black

Cheri Black (Ph.D. 1994, University of California at Santa Cruz) is no stranger to readers of *Notes on Linguistics*. Last year she wrote a series on Government and Binding Theory, which was quite helpful to many. She is a regular teacher at SIL-North Dakota in syntax and computational linguistics, and has written on languages as diverse as Kinande, Greek, and Zapotec. Cheri is currently one of the consultants for the CARLA (Computer Assisted Related Languages Adaptation) project, working on tools to assist syntactic parsing.

Leslie Bruce

After working on a New Testament in the Alamblak language of Papua New Guinea, Les Bruce got his Ph.D. from Australian National University with dissertation on the grammar of Alamblak. He has been active in linguistics consulting for a long time, and was recently the International Associate Linguistics Coordinator for SIL. Les has helped run grammar and semantics workshops in the Pacific area, India, and most recently Côte d'Ivoire and

Ghana. Currently his interests are in lexical semantics, especially in semantic primitives.

Roderic Casali

Rod Casali (Ph.D., UCLA 1995) has had an interest in linguistics from the beginning of his service with SIL. He has worked intensively in literacy and translation with the Nawuri language in Ghana, and served as Linguistics Coordinator of the Ghana Institute of Linguistics, Literacy, and Bible Translation. More recently he has become the Africa Area Linguistics Coordinator. He has coordinated several phonology workshops in West Africa, and is one of the world's leading scholars on African vowel systems.

Lou Hohulin

Lou Hohulin was an International Consultant for some years, but dropped the title when she served as my predecessor as International Linguistics Coordinator. She is one of SIL's most experienced consultants and teachers, having conducted many workshops over the years, first in the Philippines, where she was involved in two Ifugao translations, and more recently in other parts of Asia. Lou has taught Translation Principles and Field Methods in SIL schools, most recently in Dallas. Her main areas of expertise are grammar and particularly discourse.

Electronic Working Papers – call for submissions

One of the better-kept secrets of SIL is the existence of our Electronic Working Papers series (EWP). The purpose of the series is to provide an electronic medium for publishing working papers related to the work of SIL. It is part of the SIL web site, found at http://www.sil.org/silewp/. We would like to invite submissions to the series.

What is a 'working paper'? The idea of a working paper is that it is a relatively short prose document such as a conference paper or a journal article. A data corpus is not a working paper because it is not prose; and a monograph is not a working paper because it is too long. We may call a paper a 'working paper' rather than a formally published (or publishable) paper for any of several reasons:

- it may represent work in progress;
- it may be unacceptable to professional journals because it is data oriented rather than theoretical or because it represents a narrow field of interest;

- it may be a paper given orally at a conference or professional meeting;
- it may be a paper previously published but not readily obtainable.

Thus a working paper is (at least potentially!) slightly less formal than an article in *Notes on Linguistics*.

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Submission requirements. There is no standard style for the series. The only requirement is that the paper be internally consistent in following some standard academic style. All submitted papers must already be machine-readable; hard copy papers cannot be accepted. There are three categories of document format:

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Papers accepted for publication are of course still subject to editorial review and you may be asked to revise the paper. Please send your submissions for EWP to *Mike_Cahill@sil.org*.

-Michael Cahill

International Linguistics Coordinator ■

Dissertation Abstract

Grammatical Relations in Cebuano

Kari Juhani Valkama
PhD—University of Helsinki, Finland

This dissertation analyses several grammatical systems in Cebuano, a Philippine type language, spoken by over 20 million people in the Central and Southern Philippines.

First an overview of basic morphology is given to enable the reader to get a picture of the language and better follow the examples and argumentation in this study.

In the chapter on voice, it is argued that Cebuano has four grammatical voices in addition to intransitive: antipassive, active, inverse and passive. The antipassive differs from the other voices by verbal affixation. Passive and inverse voices differ from active voice by word order. Passive differs from inverse by nominal marking of the P argument.

In the chapter on case, it is argued that Cebuano is morphologically ergative.

In the chapter on orientation, the so-called focus system is analysed. The term 'orientation' is proposed as a replacement for the term 'focus'. It is argued that orientation is promotion to direct object and thus separate from the voice system. There are four orientations: actor, goal, instrumental and locative. It is argued that the choice of orientation is influenced by transitivity. Clauses with the highest transitivity are in goal focus, then instrumental focus, then locative focus, and finally actor focus, which is a derived intransitive.

In the chapter on topic it is argued that, against the common view, it is the ergative NP that is most topic worthy and thus most often is the topic.

In the chapter on grammatical relations it is argued that the ergative NP in transitive clauses and the absolutive NP in intransitive clauses is the subject, i.e., the S/A is the subject.

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■

Interpreting CECIL frames: Examples from Chimila

Terry Malone
SIL—Colombia

[**Editor's note**: This article is the second in a series of three articles by the author discussing the use of CECIL, a computer program or system used for the acoustic analysis of speech. The first article 'Is CECIL worth the bother?' is in *Notes on Linguistics* 3.1 (Malone 2000). The third, an annotated bibliography on acoustic phonetics with special relevance to CECIL, will appear in a subsequent *NOLx* issue.]

The following pages are a presentation of samples of digitized data done with SIL's CECIL system. The samples come primarily from Chimila, a Chibchan language of Northern Colombia.

Although sample digitized data were available on CECIL release disks and there were numerous figures in the accompanying documentation, when I was getting started using the program and hardware, I had many questions about the figures appearing on my computer screen. My questions simply were not resolvable using these resources. What I most desired were samples that labeled in detail what was going on in the waveform and frequency graphs. I didn't find these.

Version 3.5 of LinguaLinks (a CD released by SIL 1999) moves toward correcting this deficiency. It contains chapters 2 through 5 of a preliminary edition of *Acoustic Phonetics*, by Joan Baart. This work is designed precisely for field linguists who are not intimately familiar with acoustic phonetics and who are struggling with the interpretation of figures produced by CECIL.

The purpose of this article is to go further toward correcting the deficiency. The article presents detailed, labeled, sample CECIL diagrams, mostly from my own work on Chimila.[1] The figures illustrate some of the analytical problems described in Malone 2000, for which CECIL provided crucial illuminating data. Each figure is discussed in detail on the following pages in a way that I hope may enlighten incipient CECIL users, and encourage other field linguists to consider using CECIL in their own investigation.

[1] Data for file numbers up through 652 have been digitized using the blue box and associated program versions. After that I used WinCECIL 2.2. I used the WinCECIL 2.2 print function to print out all figures included in this article.

Figure 1: The prenasalized palatal semiconsonant. These figures, representing the word [ⁿɲjaʲʲja?] 'craw', illustrates the prenasalized palatal stop ([ⁿɲ]) preceding the alveopalatal semiconsonant [j] in word-initial position[2]. The prenasalization (A) is identical to that preceding voiced stops (compare with figure 4a), the frequency trace breaks as one would expect for stops (B), and the rest of the segment (C) is what one would expect for the alveopalatal semiconsonant in this language. The prenasalization renders the [ⁿɲj] slightly longer than one would expect for a unitary segment in word initial position in this language. This can be seen by comparison with syllable-initial [j] (D) in this same word. The lightly articulated stop between the prenasalization and the semiconsonant is not as audible here as in other examples of this segment. The onset of the semiconsonant [j] occasions a slight rise in pitch; the effect on pitch is especially clear in the raw plot for frequency.

The light palatal stop [ʲ] preceding the word medial occurrence of the semiconsonant is clearly audible. The voicing of the palatal stop appears as a short line in the break of the overall smooth frequency trace. If one listens to the articulation, it becomes clear that the preceding vowel shifts to a high vocalic glide [ʲ] which closes down into the palatal stop. Thus the voicing trace here should be more continuous than the smooth frequency plot would indicate. For this segment the raw plot actually indicates more of what is going on in the speaker's mouth. The word medial [j] is separated from the following vowel because of the laryngealization, which untypically extends through the whole length of the vowel and results in the unusually broken up wave form (E). Because of this the position of the glottal stop is approximate. CECIL data for Figure 1 are:

(1) Title: 'craw'; Length: 21117 bytes / 1.083secs; Sampling frequency: 19500Hz; Date digitized: 1 April 1996 (+f); Source: c1560.utt; Calc range: 150-340Hz; Voicing threshold: 25; Percent change: 5; Group size: 3

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[2] The semiconsonant [j] and nasal [ɲ] are alveopalatal. Unfortunately the IPA constraints under which CECIL operates, and under which it permits phonetic transcription, do not allow for a closer transcription that Americanist symbols allow for this and other alveopalatal segments. See also footnote 6.

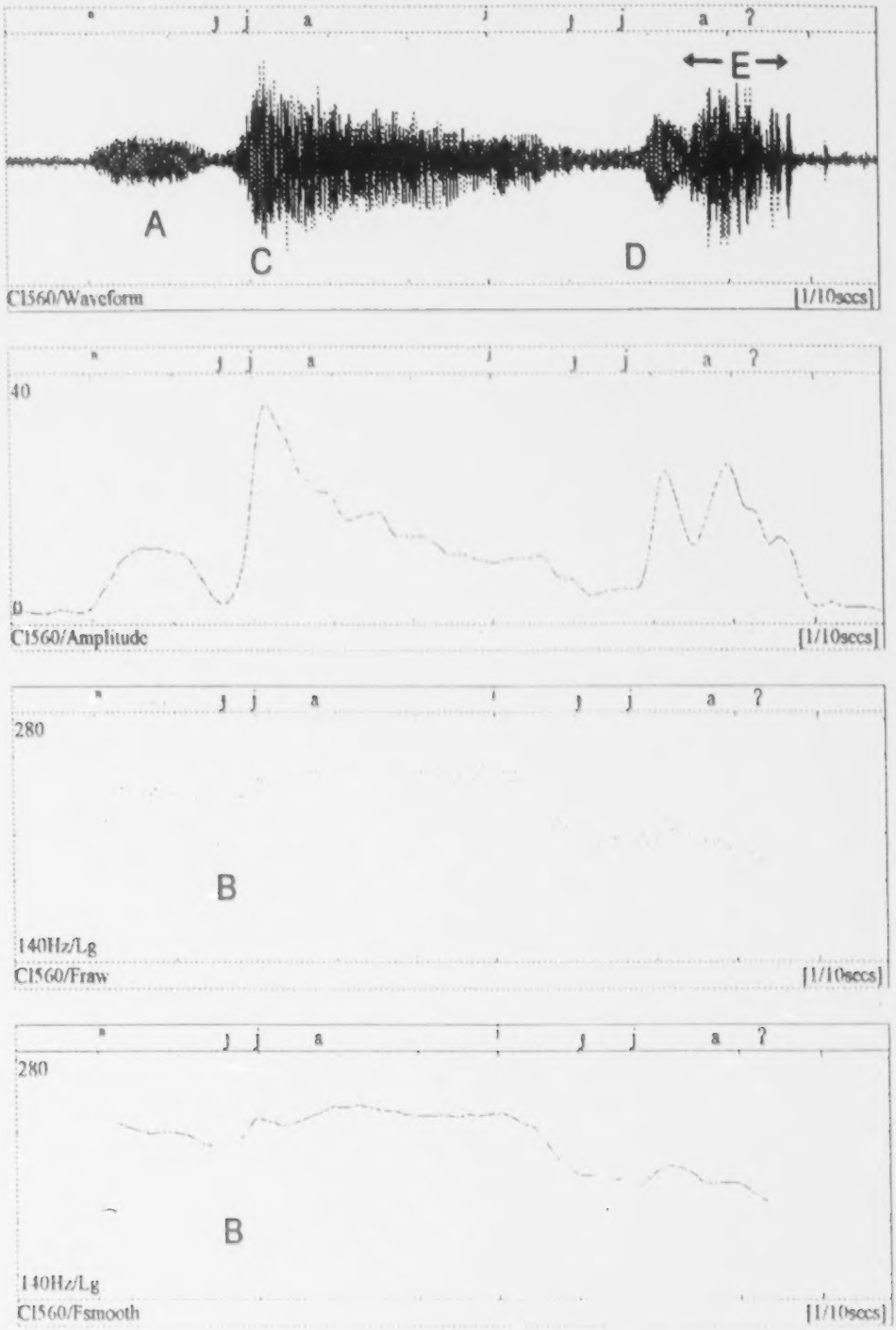

E
A
C
D
C1560/Waveform
[1/10secs]
40
0
C1560/Amplitude
[1/10secs]
280
B
140Hz/Lg
C1560/Fraw
[1/10secs]
280
B
140Hz/Lg
C1560/Fsmooth
[1/10secs]

Figure 1

Figures 2a,b,c,d,e,f: Word-medial [nn] and [nɲ]. This series of figures should give the reader some idea of how CECIL can be used as an experimental tool for determining the exact phonetic nature of segments that may not match up with what one has learned in phonetics class. Here I was trying to verify the phonetic nature of word medial [nɲ], and establish that there was indeed a contrast with word-medial syllable initial [ɲ]. CECIL data for figures 2a and 2b are:

(2a) Length: 17144 bytes / 0.879secs; Sampling frequency: 19500Hz; Date digitized: 19 November 1990 (+f); Source: CL106.utt; Calc range: 180-320Hz; Voicing threshold: 25; Percent change: 5; Group size: 5

(2b) Length: 12531 bytes / 0.643secs; Sampling frequency: 19500Hz; Date digitized: 19 November 1990 (+f); Source: CL105.utt; Calc range: 180-320Hz; Voicing threshold: 15; Percent change: 5; Group size: 3

It was necessary to check the characteristics of the dental and palatal nasals in word-initial position, in order to have a standard of comparison. Figures 2a [na·] 'a personal name' and 2b [ɲʲaʔa] 'ok, let's go!' illustrate the dental nasal (A) and the alveopalatal nasal plus offglide (B and C) when these occur in word initial position.

When I replayed the segment BC of figure 2b in CECIL, section B sounded identical to the dental nasal A of figure 2a. Section C sounded and looked like an alveopalatal semiconsonant, except it was a little shorter than one would expect. The result of combining B and C was an alveopalatal nasal that was longer than one would normally expect for a unitary segment.

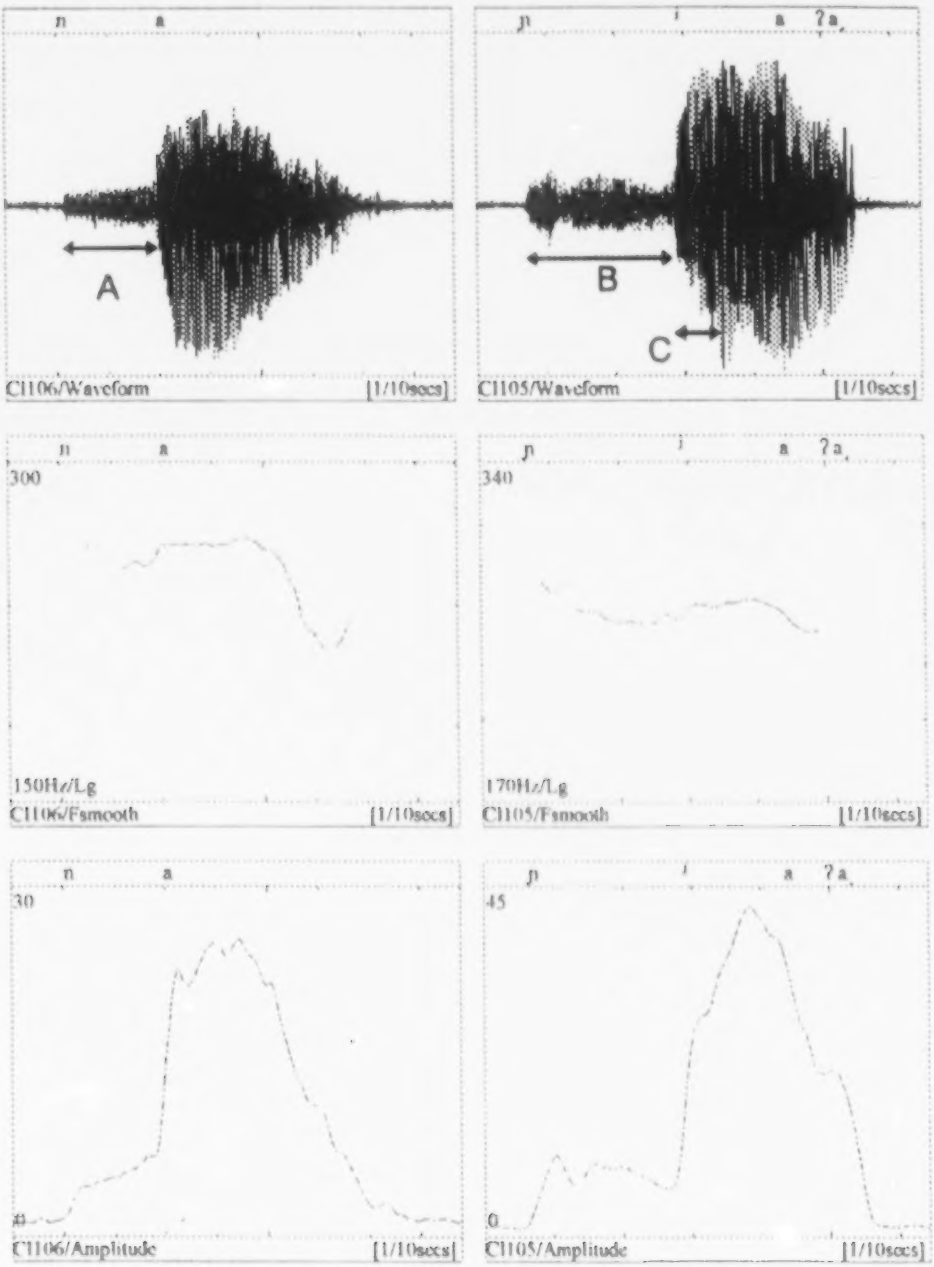


Figure 2a

Figure 2b

As a next step I compared the segment BC with a palatal nasal plus offglide in a language where it functions as a unitary segment. Although the palatal nasal is rare in word initial position in Spanish, it does occur word medially and functions as a unitary segment in the syllable onset; figure 2c illustrates this segment (A) in the Spanish word [kaɲʲa] 'sugar cane', as pronounced on the northern coast of Colombia. This segment is .152 second in length, and the off-glide [ʲ] (B) is .043 second or less. CECIL data for this figure are:

(2c) Length: 10876 bytes / 0.643secs; Sampling frequency: 19500Hz; Date digitized: 29 July 1992 (+f); Source: SP3.utt; Calc range: 150-300Hz; Voicing threshold: 5; Percent change: 5; Group size: 2

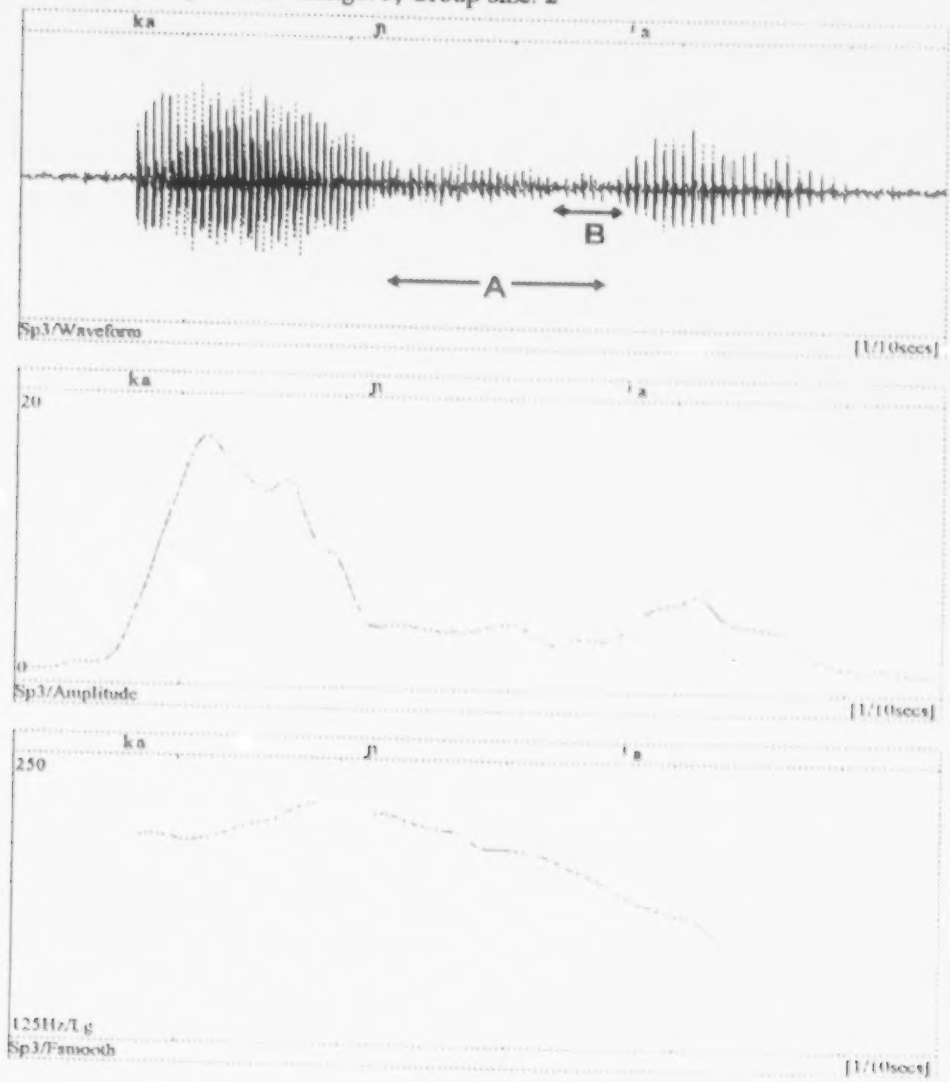


Figure 2c

In figure 2d this nasal (A) in syllable initial position in the Chimila word [kra·ɲʲari] 'clear' (liquid) is .168 second in length, and the off-glide (B) takes up .077 second of this total. Unlike Spanish, the nasal consonant influences the preceding vowel noticeably (C). The replay function reveals that this is indeed a nasalized vowel, and not part of the nasal consonant. These measurements, comparisons, graphs, comparative evidence, and distribution supported my conclusion that the [ɲ] was a surface phonetic representation of the sequence /nj/. CECIL data for this figure are:

(2d) Title: 'clear (liquid)'; Length: 20878 bytes / 1.071secs; Sampling frequency: 19500Hz; Date digitized: 13 March 1993 (+f); Source: CL311.utt; Calc range: 50-400Hz; Voicing threshold: 20; Percent change: 5; Group size: 3

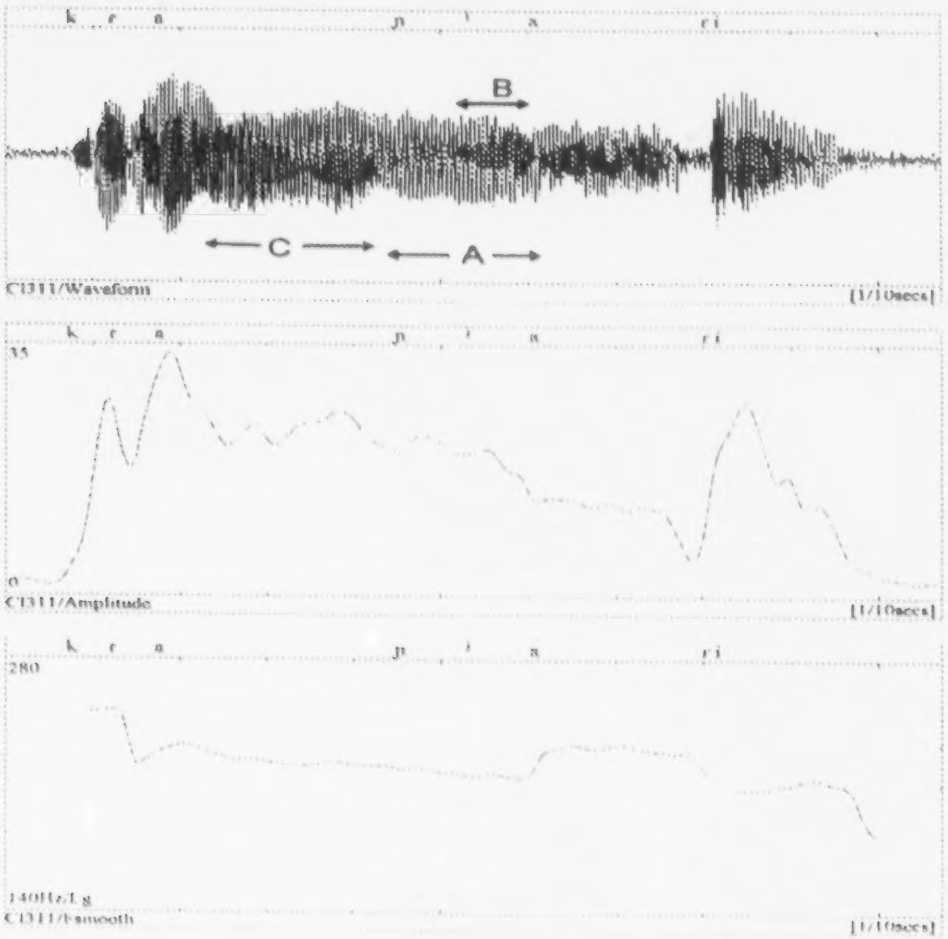


Figure 2d

Curiously, a word similar to the Spanish word of figure 2c occurs in Chimila, [kanɲʲa] 'cultivated field' (figure 2e), for which CECIL data are:

(2e) Title: field; Length: 18584 bytes / 1.071secs; Sampling frequency: 28000Hz; Date digitized: 15 November 1990 (+f); Source: CL88.utt; Calc range: 220-310Hz; Voicing threshold: 13; Percent change: 5; Group size: 3

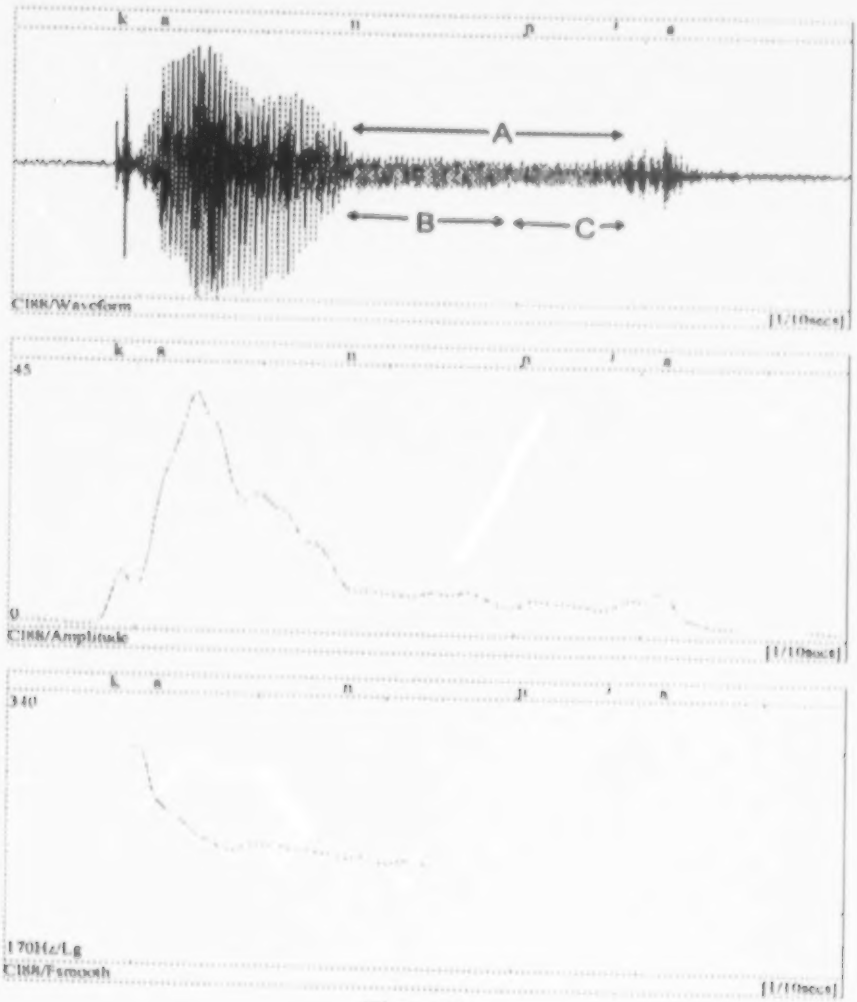


Figure 2e

This word contains word medial [nɲ] (A). The replay function clearly distinguishes the dental nasal section (B) from the palatal nasal section (C). To some degree the difference appears in the waveform. In some words with this sound the boundary is not so clear. The length of the total is that of two successive resonants, or a double consonant. This can be seen by comparing

with the long dental nasal [n·] (A) in figure 2f, [han·akʌ] 'trickling out of a container', for which CECIL data are:

(2f) Length: 25511 bytes / 0.911secs; Sampling frequency: 28000Hz; Date digitized: 15 November 1990 (+f); Source: CL89.utt; Calc range: 220-310Hz; Voicing threshold: 10; Percent change: 5; Group size: 3

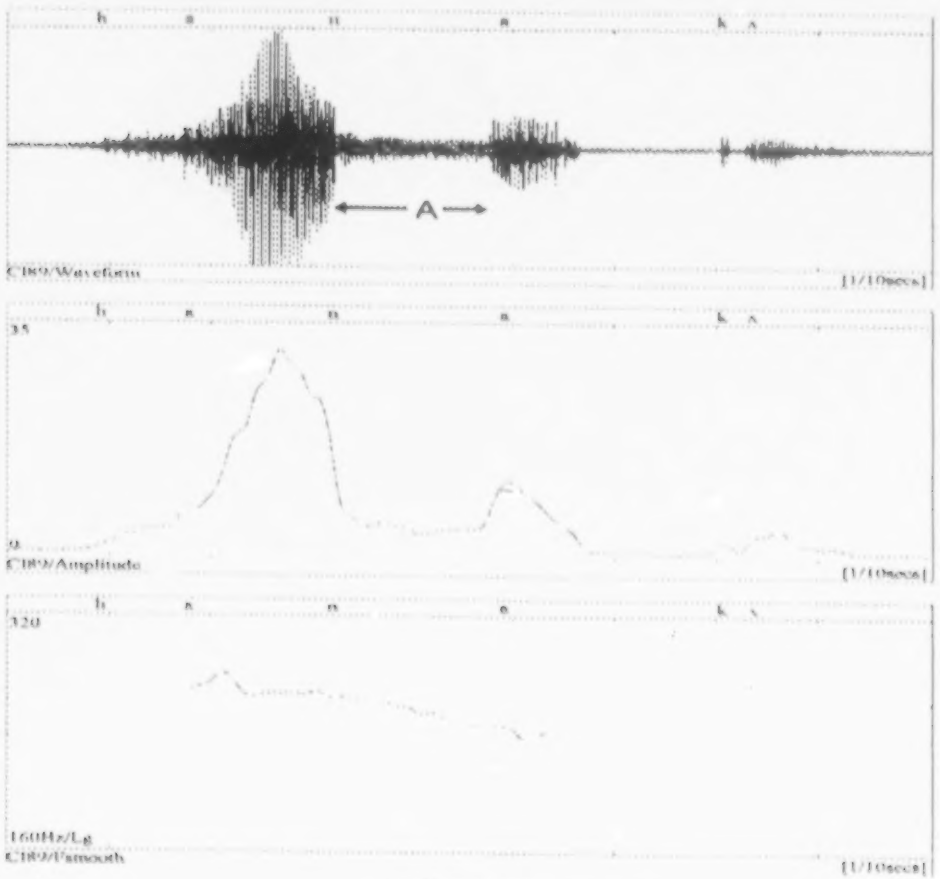
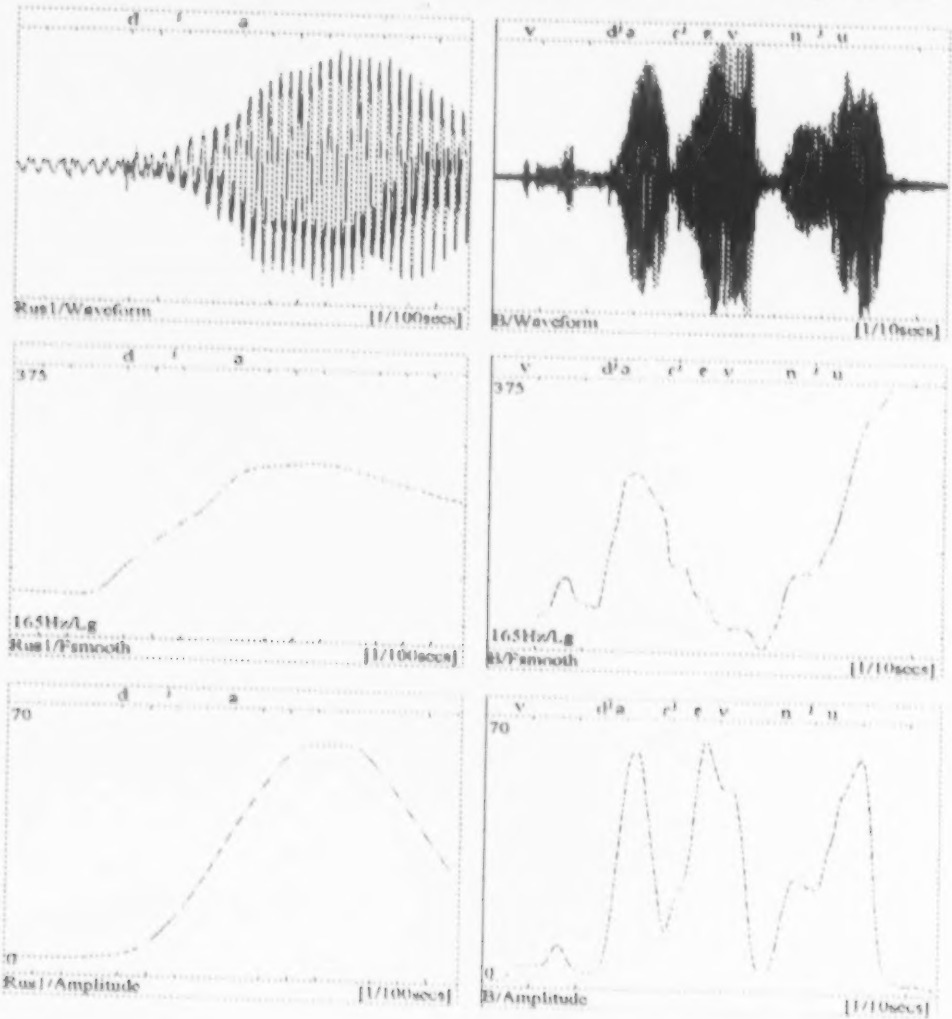


Figure 2f

In some words containing [nɲ] the alveopalatal nasal sounds more like an alveopalatal semiconsonant. This, along with evidence such as that in figures 2d,e,f, and distributional irregularities eventually led to the conclusion that [nɲ] was a word medial allophone of the complex segment [ⁿʝj]; i.e. a word medial version of the sequence /dj/ in which the prenasalization of the dental stop had pasted itself to the coda of the previous syllable. When it finally occurred to me to ask my language consultant to pronounce the word of figure 2e in slow speech, syllable by syllable, she produced the successive syllables [kaʰ] and [ⁿʝja].

Figures 3a,b: Palatalization as an articulatory modification. In section 3.2 of Malone 2000 I claim that the palatal segment in the phonetic realizations of the clusters /tj/, /dj/ and /nj/ is longer than one would expect for palatalization as an articulatory modification. The comparison with syllable initial /j/ in figure 1 and /ɲ/ in figure 2d helps bear this out. It has also been helpful to compare with a language in which palatalization occurs as an articulatory modification.

Figures 3a,b represent the digitized version of the Russian phrase [v dʲərʲɛvnʲu] 'in (the) village'.[3] CECIL data for both of these figures are:



[3] The data come from the recording which accompanies Michailoff 1986.

Figure 3a

(3a,b) Title: in (the) village; Length: 21203 bytes / 0.879secs; Sampling freq: 22050Hz; Date digitized: 13 August 1998 (+f); Source: rus1.utt; Calc range: 40-500Hz; Voicing threshold: 20; Percent change: 10; Group size: 6

In each figure the full phrase appears on the right-hand side. Figure 3a shows an enlargement of the syllable [dʲʊ] on the left-hand side; figure 3b shows an enlargement of the syllable [nʰu] on the left-hand side. In each case the palatalization is a third or less as long as the palatal segment of Chimila /dj/ and /nj/, respectively.

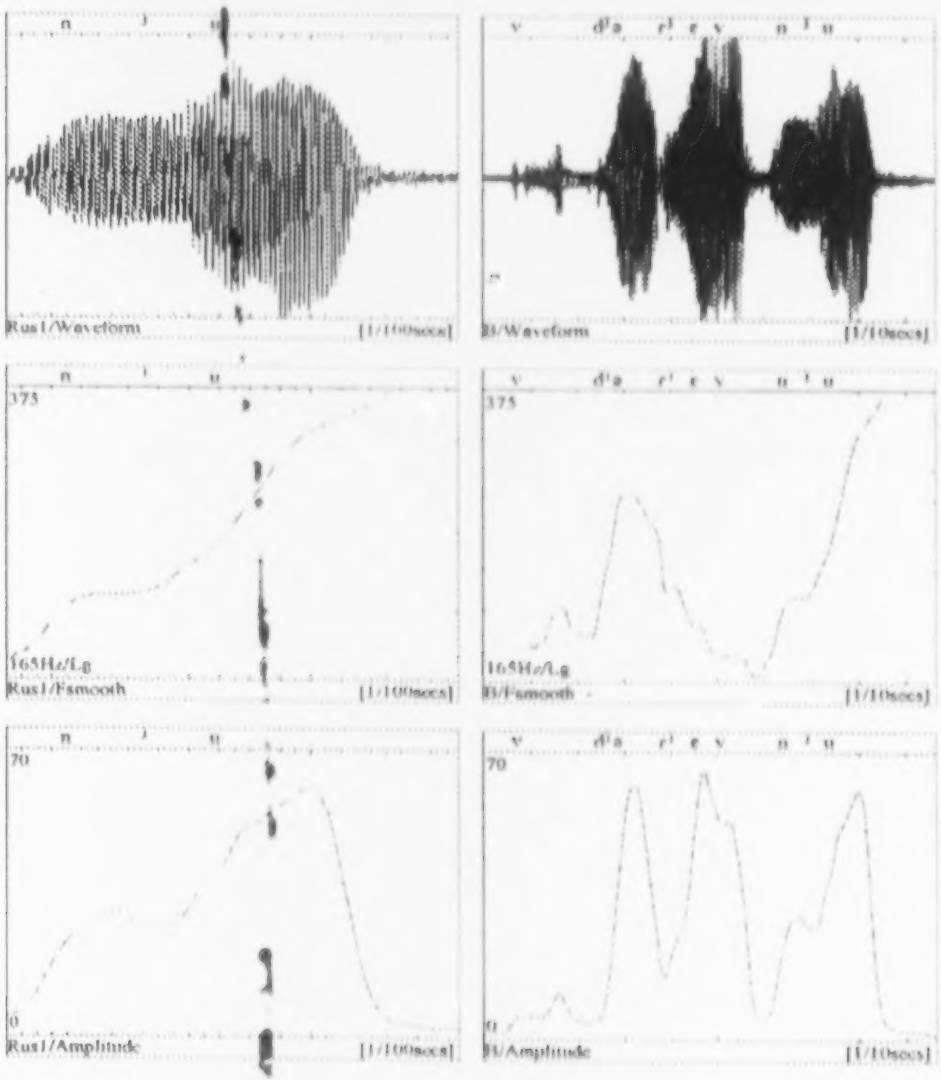


Figure 3b

Figures 4a,b,c: Fully articulated nasals preceding prenasalized stops. The figures 4a,b and c contrast vowel medial prenasalized voiced stops with sequences of full nasal consonants and prenasalized voiced stops. In figure 4a [$tu^{h}{}^{m}bri$] 'drum' the glottal off-glide [^{h}] closes the first syllable (A). No nasal consonant is present, and the prenasalization (B) immediately precedes the bilabial voiced stop (C) as one would expect. CECIL data for this figure are:

(4a) Title: 'drum'; Length: 20289 bytes /1.040secs; Sampling frequency: 19500Hz; Date digitized: 27 August 1992 (+f); Source CL165.utt; Calc range: 200-350Hz; Voicing threshold: 20; Percent change: 5; Group size: 3

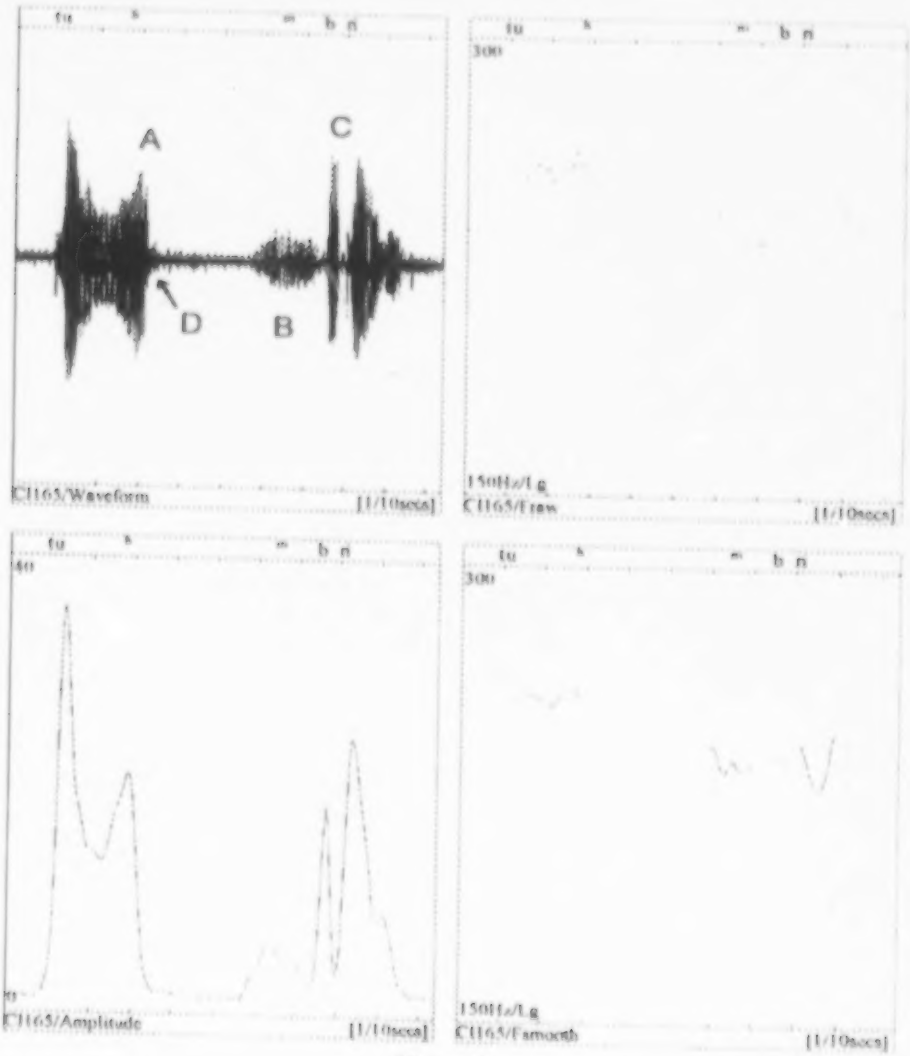


Figure 4a

No full nasal consonant closes the first syllable in figure 4b [ti·ᵐbru] 'wasp', for which CECIL data are:

(4b) Title: 'wasp'; Length 15900 bytes / 0.815secs; Sampling frequency: 19500Hz; Date digitized: 10 May 1996 (+f); Source: CL222.utt; Calc range: 150-300Hz; Voicing threshold: 15; Percent change: 5; Group size: 3

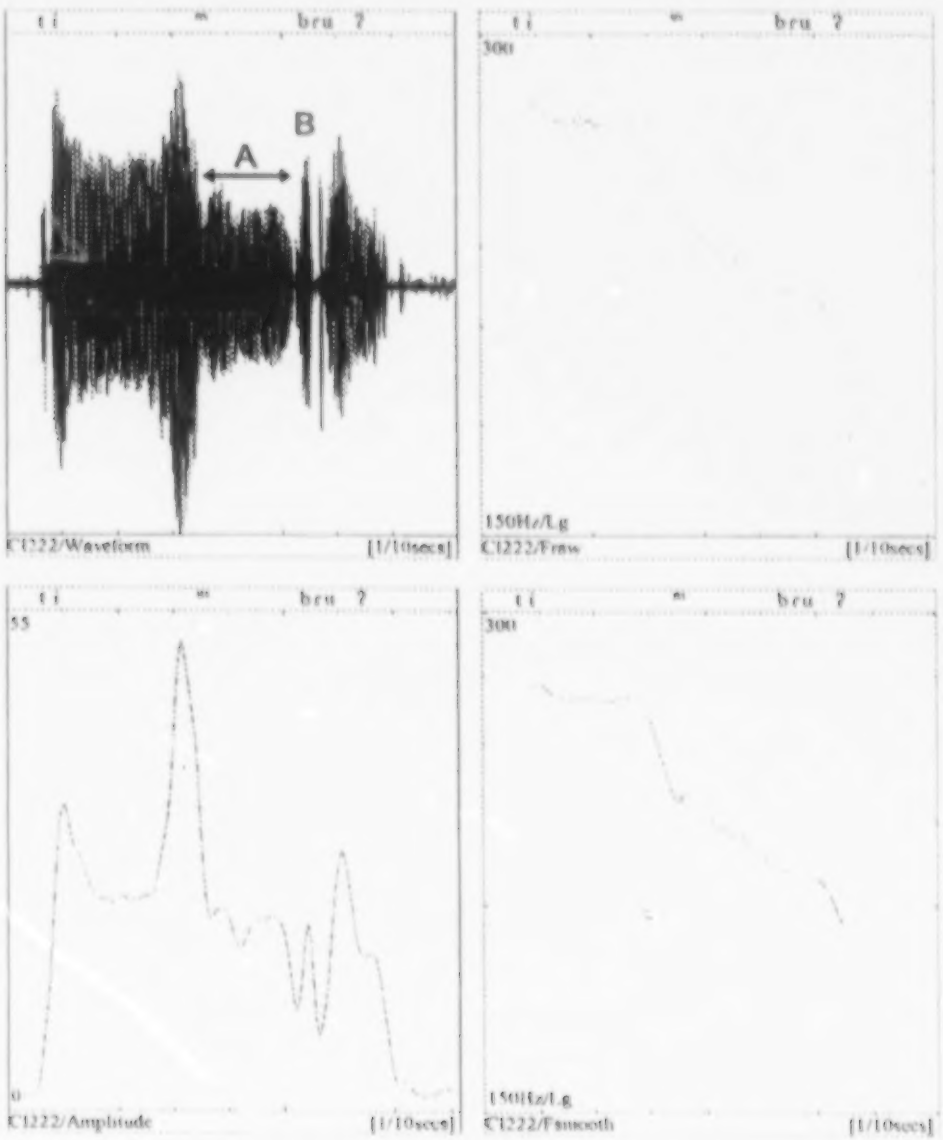


Figure 4b

The nasal (A) in 4b represents the prenasalization of the bilabial voiced stop (B). The prenasalization of this stop has pasted itself to the coda of the previous syllable in observation of a phrase level mora insertion rule (the word was the second of two isolated articulations which together received sentence level intonation, as described in section 2, Malone 2000).

In contrast, a full nasal consonant (A) closes the first syllable in figure 4c [tim^{m}bri?] 'oblong packet of boiled corn mush'. The CECIL data are:

(4c) Title: 'oblong packet of boiled corn mush'; Length 16080 bytes / 0.825secs; Sampling frequency: 19500Hz; Date digitized: 27 August 1992 (+f); Source: CL166.utt; Calc range: 150-350Hz; Voicing threshold: 15; Percent change: 5; Group size: 3

The prenasalization (B) of the voiced bilabial stop immediately follows. (C) designates a possible transitional area between A and B. Even so, the boundary between the two zones is reasonably obvious. The lengths alone of the nasal consonant and the prenasalization provide telltale evidence for the presence of both: the length of the full consonant is similar to that for other nasal consonants in syllable final position; in addition, the length of the prenasalization is similar to that observed in figures 4a and 4b (as well as prenasalization in most of my data).

In figure 4a a slight echo (D) appears at the termination of the glottal off-glide. A similar echo appears to varying degrees in other figures in this article (and in most of my data) at the end of segments which close a syllable. When measuring vowel lengths, I consider the point where the echo begins to be the terminus of the vowel. If the echo appears at the closure of the first mora of a long consonant (see figure 5a, left hand word) I measure the length of the long consonant from the beginning of the echo to the consonant release at the beginning of the next syllable. The echo appears in all versions of CECIL that I have used, and is worse in inferior recordings.

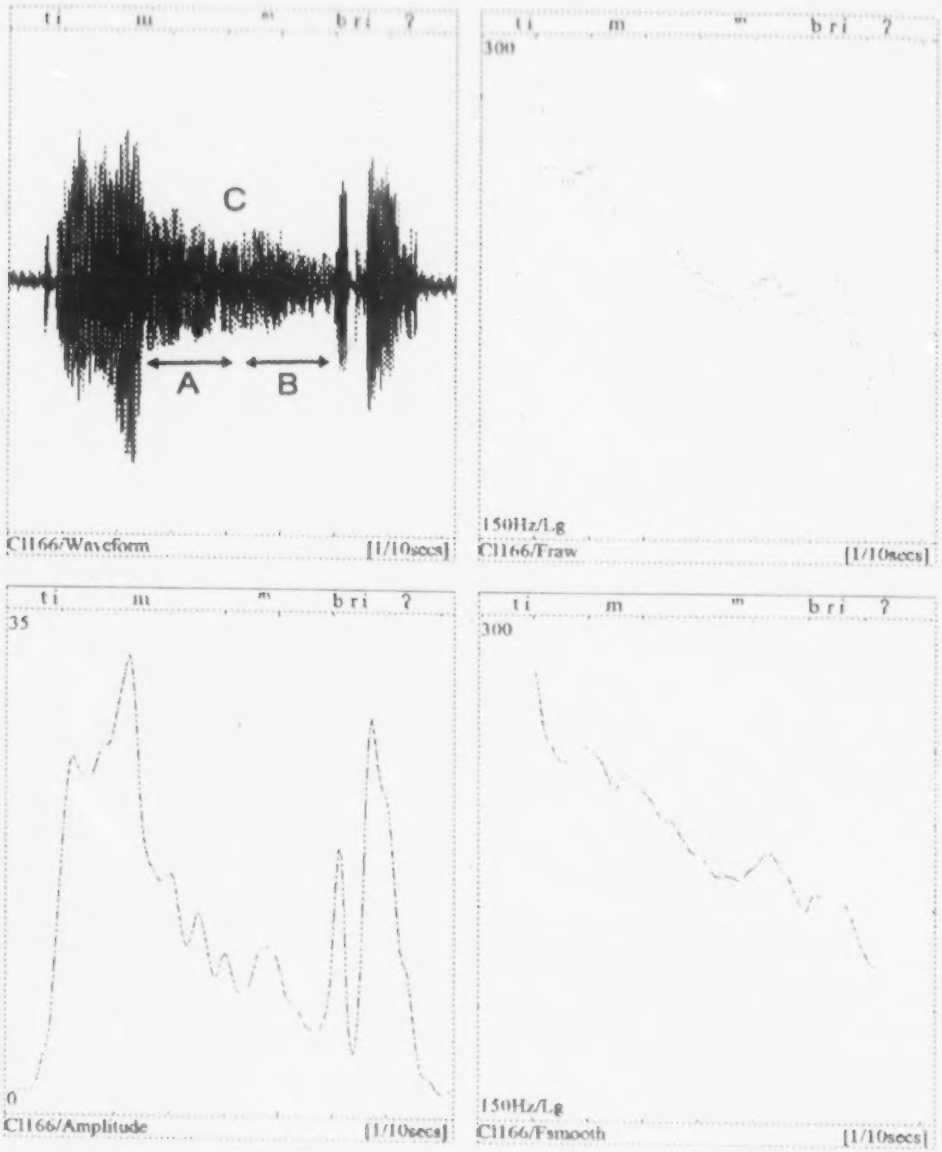

C
A
B
C1166/Waveform
[1/10secs]
300
150Hz/Lg
C1166/Fraw
[1/10secs]
35
0
C1166/Amplitude
[1/10secs]
300
150Hz/Lg
C1166/Fsmooth
[1/10secs]

Figure 4c

Figures 5a,b,c,d: Long consonants. Figure 5a contrasts the two classes of long consonants which occur in disyllabic words. On the left-hand side appears [sak·ʷi] 's/he ground' in which a two-mora long consonant occurs (A), and on the right appears [sak:ʷi] 's/he tamped down/smashed' in which a three mora consonant occurs (B). The third mora in the word 's/he ground' results from phrase level mora insertion. CECIL data for this figure are:

(5a) Left Title: 's/he ground'; Length: 25400 bytes / 1.303secs; Sampling frequency: 19500Hz; Date digitized: 30 August 1994 (+f); Source: CL505.utt; Calc range: 150-300Hz; Voicing threshold: 25; Percent change: 11; Group size: 5

Right Title: 's/he tamped down/smashed'; Length: 26611 bytes / 1.365secs; Sampling frequency: 19500Hz; Date digitized: 30 August 1994 (+f); Source: CL506.utt; Calc range: 150-300Hz; Voicing threshold: 25; Percent change: 11; Group size: 5

The two words were recorded in sequence; the speaker, who was fully aware of the difference between the two words, exaggerated the length of the longer consonant in 's/he tamped down/smashed'. The exaggerated pronunciation raised the amplitude trace in the second syllable more than is normal for disyllabic words. A more common pattern would be that of the disyllabic word in figure 5d. It also resulted in the rightward shift of the pitch-accent nucleus, accounting for the unusual intensity patterns. (Figure 4b is a good illustration of the most common, normal pitch and intensity pattern for disyllabic words.)

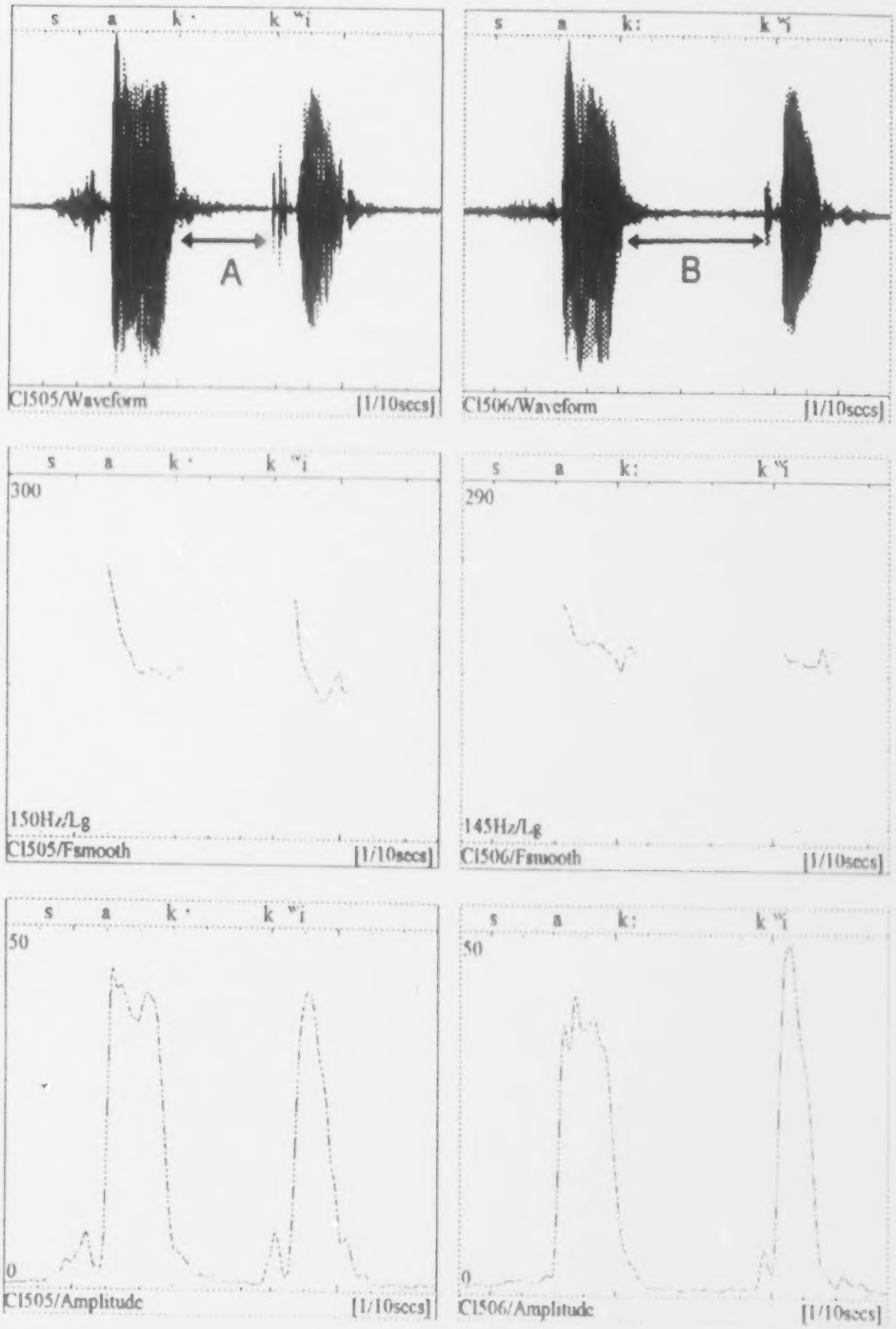

s a k· kʷi
A
C1505/Waveform [1/10secs]
s a k: kʷi
B
C1506/Waveform [1/10secs]
300
150Hz/Lg
C1505/Fsmooth [1/10secs]
290
145Hz/Lg
C1506/Fsmooth [1/10secs]
50
0
C1505/Amplitude [1/10secs]
C1506/Amplitude [1/10secs]

Figure 5a

Figure 5b compares the length of these consonants with the length of a normal gap between syllables. The word [sa:kʷi] 'man' appears on the left side; [sak·ʷi] 's/he ground' appears on the right side. The gap between the end of the long vowel and the velar stop on the left hand side (A) is .172 second, whereas the gap between the closure and the release of the long consonant on the right-hand side (B) is .266 second. CECIL data for the right side are the same as 5a left. Data for the left column of 5b are:

(5b) Left Title: 'man'; Length: 20282 bytes / 1.040secs; Sampling frequency: 19500Hz; Date digitized: 30 August 1994 (+f); Source: CL507.utt; Calc range: 150-300Hz; Voicing threshold: 25; Percent change: 11; Group size: 5

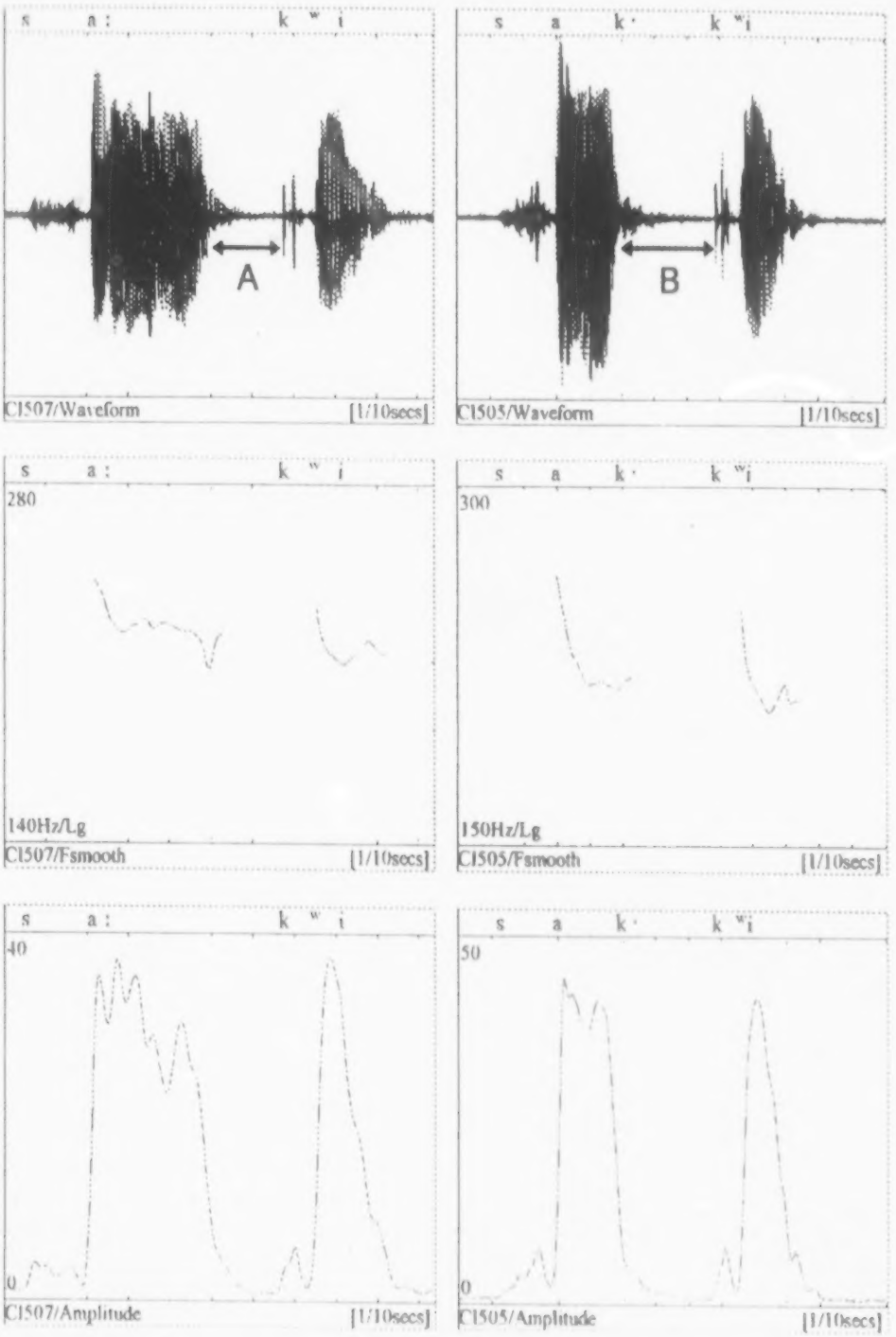

s a: k w i
A
C1507/Waveform [1/10secs]
s a k· k w i
B
C1505/Waveform [1/10secs]
s a: k w i
280
140Hz/Lg
C1507/Fsmooth [1/10secs]
s a k· k w i
300
150Hz/Lg
C1505/Fsmooth [1/10secs]
s a: k w i
40
0
C1507/Amplitude [1/10secs]
s a k· k w i
50
0
C1505/Amplitude [1/10secs]

Figure 5b

Figure 5c compares the gap between a vowel-final CCV syllable with the length of a long consonant in the same position in two trisyllabic words. The word [kʷakata] 'pour (it) out!' appears on the left hand side, and the word [ᵍwittoka] 'put the point of the (vertical) pole to one side' is on the right hand side. The gap between the end of the one mora vowel in 'pour (it) out!' (A) is .162 second, whereas the gap between the closure and release of the consonant on the right-hand side (B) is .251 second. Only one degree of consonant length appears in trisyllabic (or longer) words.[4] CECIL data for this figure are:

(5c) Title: 'pour (it) out!'; Length: 23062 bytes / 1.303secs; Sampling frequency: 19500Hz; Date digitized: 27 October 1994 (+f); Source: CL524.utt; Calc range: 150-300Hz; Voicing threshold: 20; Percent change: 6; Group size: 5

Title: 'put the point...'; Length: 37419 bytes / 3.394secs; Sampling frequency: 11025Hz; Date digitized: 25 October 1999 (+f); Source: CL703.utt; Calc range: 40-500Hz; Voicing threshold: 20; Percent change: 10; Group size: 6

Note that normally one should compare words with consonants at the same point of articulation, and vowels at the same height. However, voiceless stops [t] and [k] behave similarly with respect to length in this position elsewhere in my data, and the difference in point of articulation of these two segments does not seem to significantly alter length measurements or shortening behaviors. Elsewhere in my data vowel height does not seem to significantly affect length measurements or shortening behaviors of contiguous consonants.[5]

[4] The waveform between the last two syllables of [ᵍwit oka] represents the chirp of a bird that unfortunately ended up on this particular recording.

[5] My choice of examples here in part depends on what I have happened to record on disk of the more than 3000 words and phrases that I have digitized; I did not have in mind using examples in papers when I first starting using CECIL.

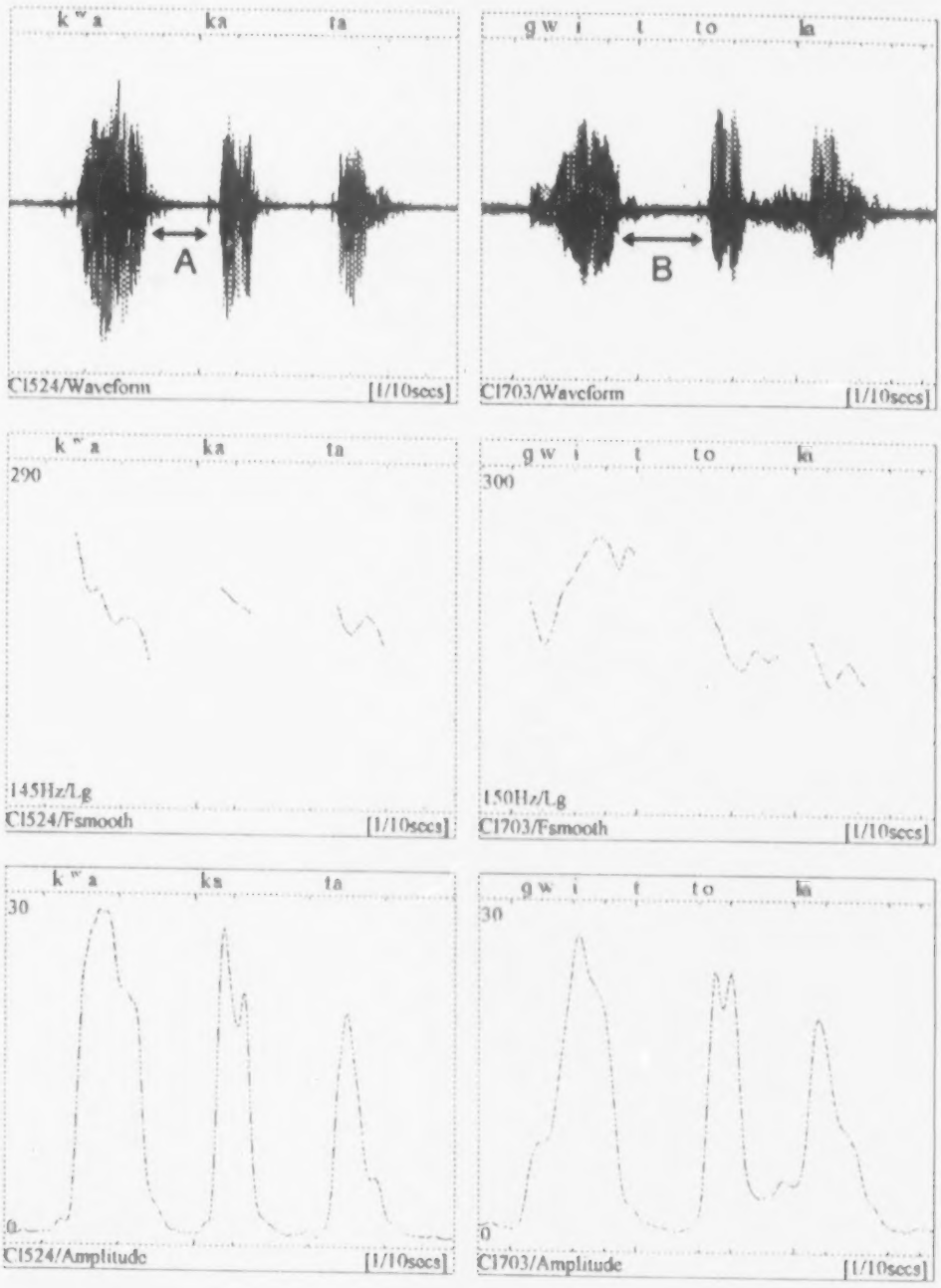

k ʷ a
ka
ta
A
C1524/Waveform
[1/10secs]
g w i
t
to
la
B
C1703/Waveform
[1/10secs]
290
145Hz/Lg
C1524/Fsmooth
[1/10secs]
300
150Hz/Lg
C1703/Fsmooth
[1/10secs]
30
0
C1524/Amplitude
[1/10secs]
C1703/Amplitude
[1/10secs]

Figure 5c

The relative lengths of long consonants decrease in proportion to the increase of syllables in the word, as one might expect. Figure 5d compares the gap between a vowel-final CCV syllable with the length of a long consonant in the same position. CECIL data for this (5d) are:

(5d) Title: 'pour out!'; Length: 33600 bytes / 3.048secs; Sampling frequency: 11025Hz; Date digitized: 25 October 1999 (+f); Source: CL702.utt; Calc range: 40-500Hz; Voicing threshold: 20; Percent change: 10; Group size: 6

Title: 'put the point...'; Length: 37419 bytes / 3.394secs; Sampling frequency: 11025Hz; Date digitized: 25 October 1999 (+f); Source: CL703.utt; Calc range: 40-500Hz; Voicing threshold: 20; Percent change: 10; Group size: 6

The word [ᵍwit:a] 'pour out!' is shown on the left in Figure 5d, and the word [ᵍwit·oka] 'put the point of the (vertical) pole to one side!' is on the right. The gap between the closure of the stop and its release in 'pour out!' (A) is .402 second, whereas the gap between the closure and release of the consonant on the right-hand side (B) is .240 second.[6]

This series of figures clearly demonstrates the local effects of some segments on absolute frequency and intensity: the sibilant [s] raises both; voiceless stops raise them; consonant lengthening usually raises them slightly. The effects are minor, and do not significantly alter the overall patterns: normally frequency and intensity drop from high to low. This overall high to low pattern occurs in my data for this class of trisyllabic words, no matter what segments they contain. In fact, a comparison of the two words in figure 5c suggests that something other than segment class controls the overall frequency and intensity patterns. For example, if the overall patterns were due only to segment variations, the frequency trace for the middle syllable of [kʷakata] would be lower than those for [ᵍwit·oka], given that the vowel [o] should exhibit a slightly higher intrinsic pitch than the vowel [a] (Laver 1994:454-5).

[6] The two words translated 'pour out' refer to pouring water, but differ with respect to secondary meaning: [kʷakata] could alternatively be translated 'remove to another place!', and [ᵍwit:a] 'shift away from vertical position!'. I am using superscript g [ᵍ] to indicate light articulation. The constraints of IPA transcription in CECIL forces the segment [ᵍw] to be transcribed as [gw] in Figure 5d.

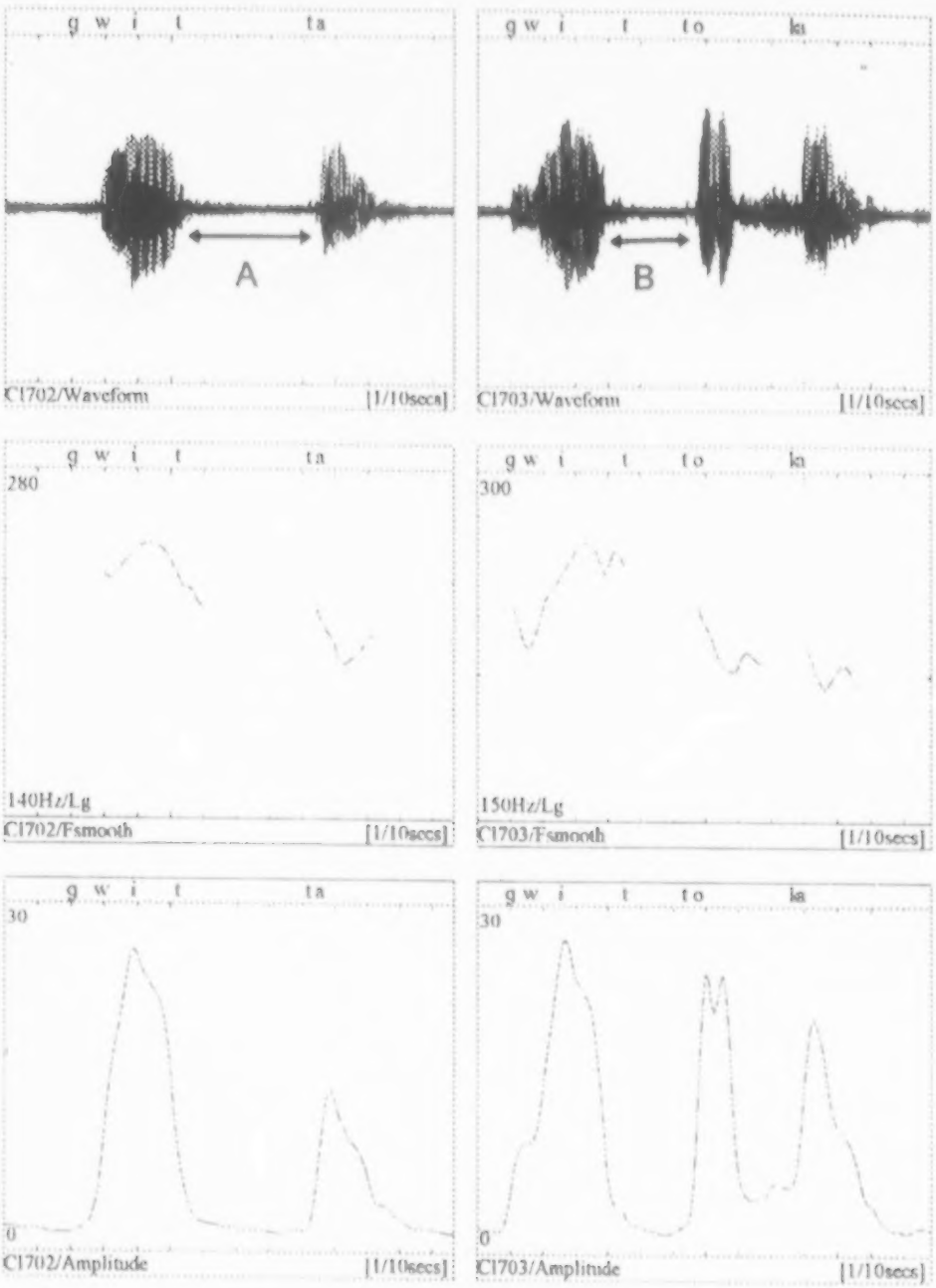


Figure 5d

Figures 6a,b,c,d: Pitch and stress operate independently. The figures in 6 are trisyllabic words with a typical stress pattern and MHL pitch. Primary

stress occurs on peak A for all four words. The frequency trace (lowest frame) clearly reveals the MHL pitch pattern. Although segment types and length condition minor variations in the pitch and amplitude traces, they do not condition the overall pitch and stress pattern.

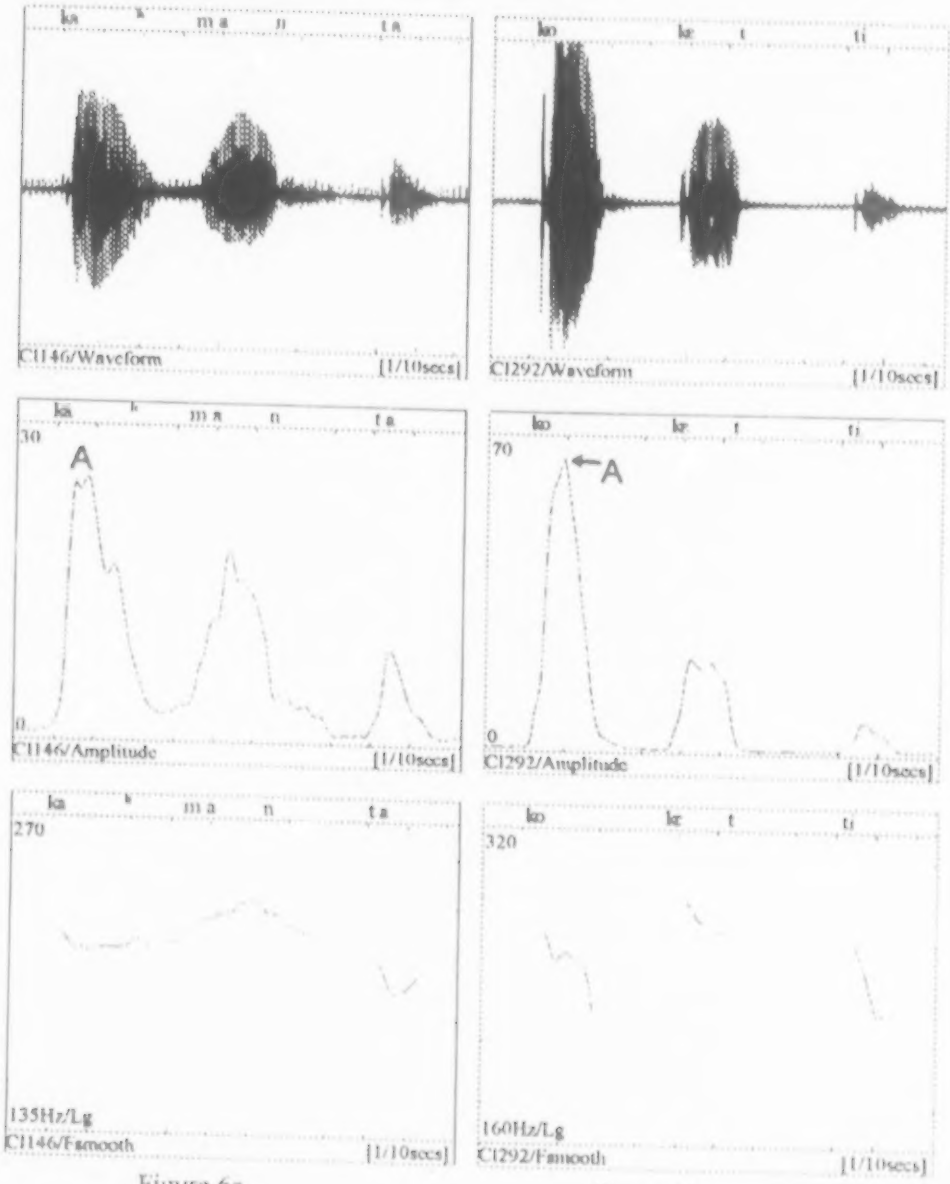


Figure 6a

Figure 6b

This is seen by comparing figures 6a [kaʰmanta] 'mother', 6b [kokɛt·i] 'slow!', 6c [ʔe·kat·e] 's/he/it is there', and 6d [ⁿdoŋ·oʔo] 'swallow it!', all with pitch pattern MHL, primary stress on the first syllable and pitch-accent nucleus on the second syllable.

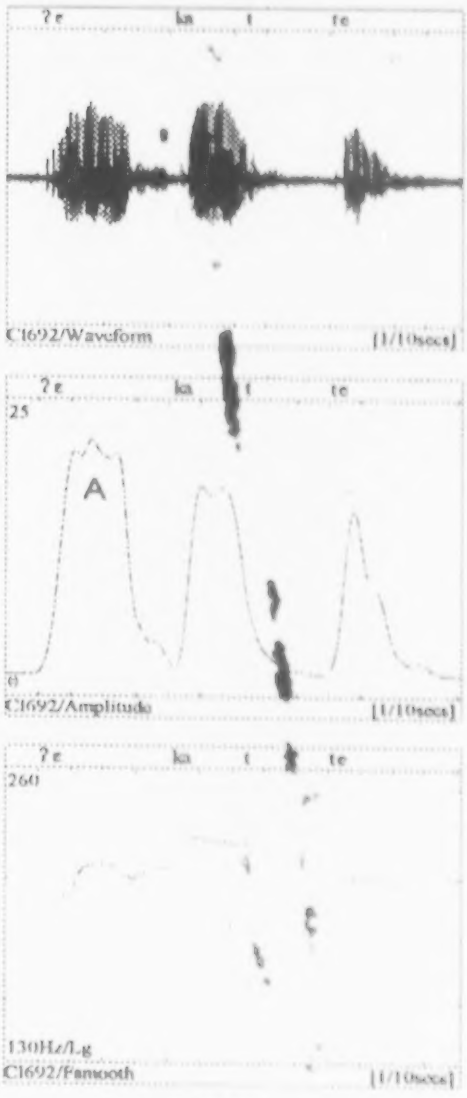


Figure 6c

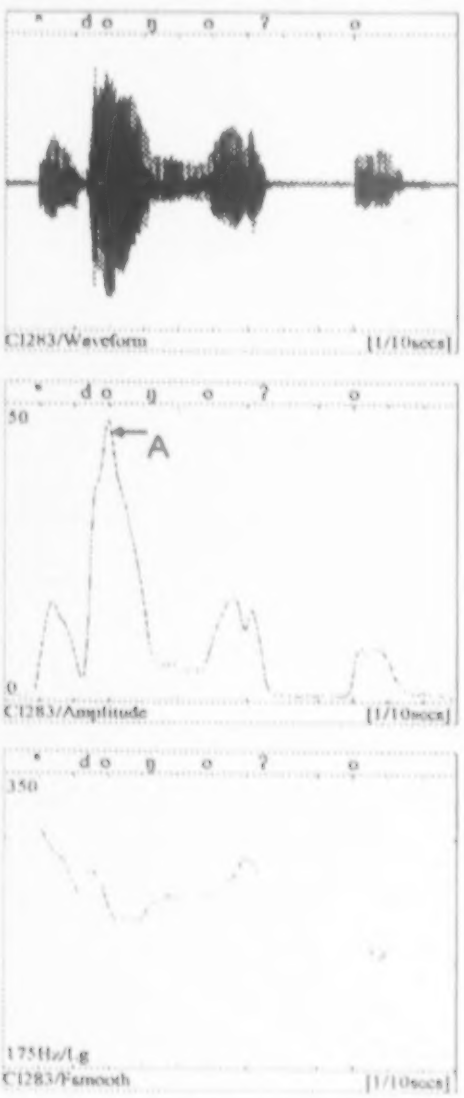


Figure 6d

CECIL data for the figures in (6) are:

(6a) Title: 'mother'; Length: 22080 bytes / 1.132secs; Sampling frequency: 19500Hz; Date digitized: 23 July 1992 (+f); Source: CL146.utt; Calc range: 150-300Hz; Voicing threshold: 18; Percent change: 7; Group size: 3

(6b) Title: 'slow!'; Length: 22455 bytes / 1.152secs; Sampling frequency: 19500Hz; Date digitized: 30 November 1992 (+f); Source: CL292.utt; Calc range: 150-350Hz; Voicing threshold: 20; Percent change: 5; Group size: 3

(6c) Title: 's/he/it is there'; Length: 34500 bytes / 3.129secs; Sampling frequency: 11025Hz; Date digitized: 25 October 1999 (+f); Source: CL692.utt; Calc range: 40-500Hz; Voicing threshold: 20; Percent change: 10; Group size: 6

(6d) Title: 'swallow it!'; Length: 25218 bytes / 1.293secs; Sampling frequency: 19500Hz; Date digitized: 30 November 1992 (+f); Source: CL283.utt; Calc range: 150-350Hz; Voicing threshold: 20; Percent change: 5; Group size: 3

Figures 7a,b,c: Pitch and intensity patterns on monosyllabic bimoraic vowels. Figure 7a represents the word [pu·ŋo?o?] 'soak the corn!', for which CECIL data are:

(7a) Title: 'moje el maíz!'; Length: 18744 bytes / 0.961secs; Sampling frequency: 19500Hz; Date digitized: 26 November 1992 (+f); Source: CL212.utt; Calc range: 150-300Hz; Voicing threshold: 15; Percent change: 5; Group size: 3

The vowel [u·] is monosyllabic and bimoraic, with highest intensity on the first mora. Pitch holds more or less level until the onset of the velar nasal. I placed the velar nasal according to where I heard its initiation on replay. The frequency and amplitude traces suggest that its onset could occur previous to where I have placed it. The boundary between the high back vowel and the velar nasal in Chimila is notoriously difficult to determine. In my data a section of waveform usually occurs between the two which sounds like a nasalized high back vowel, and which is impossible to assign to either segment. In such cases I rely on the replay function and intuition of the Chimila speaker with regard to consonant length.

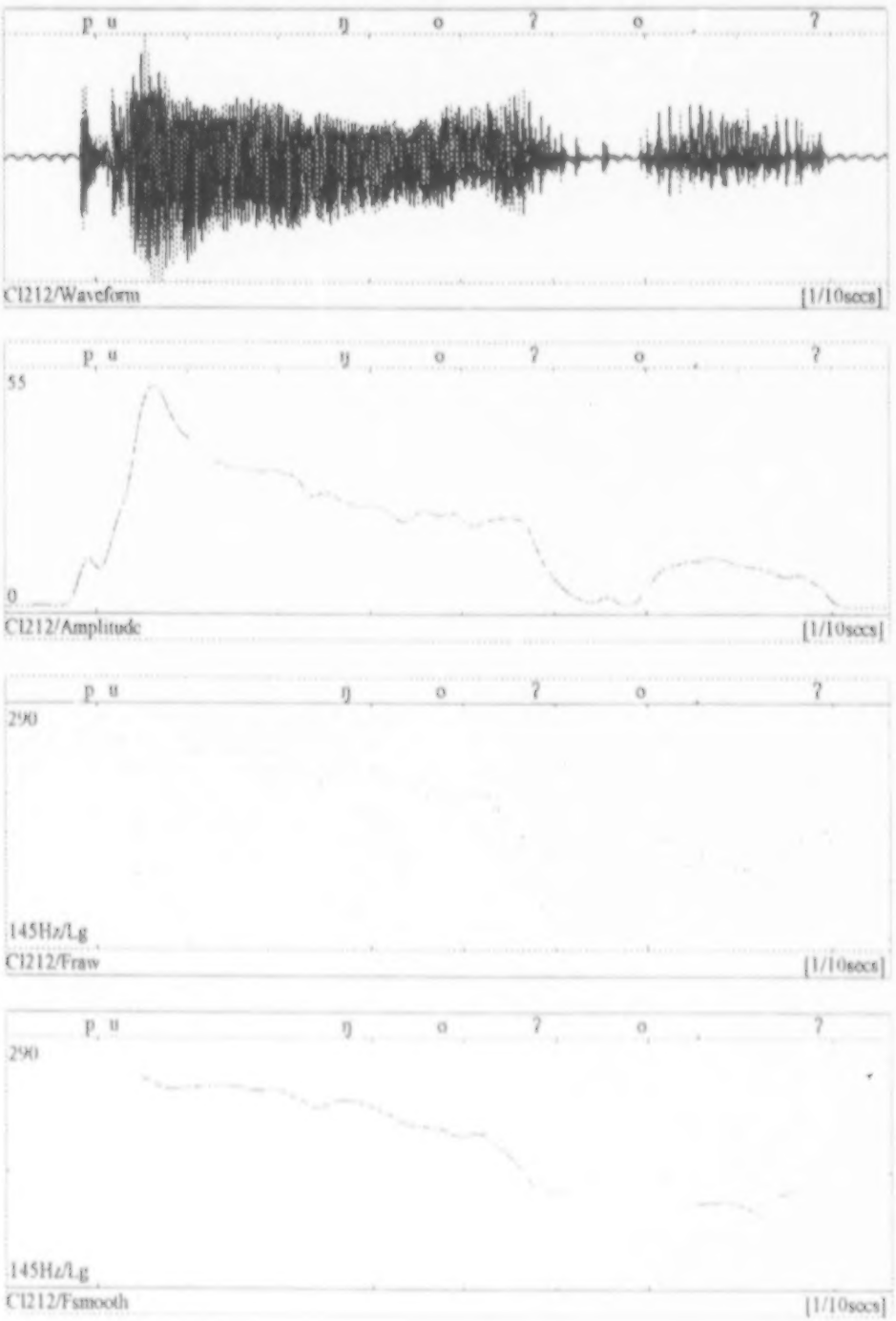

p u ŋ o ʔ o ʔ
Cl212/Waveform [1/10secs]
p u ŋ o ʔ o ʔ
55
0
Cl212/Amplitude [1/10secs]
p u ŋ o ʔ o ʔ
290
145Hz/Lg
Cl212/Fraw [1/10secs]
p u ŋ o ʔ o ʔ
290
145Hz/Lg
Cl212/Fsmooth [1/10secs]

Figure 7a

Figure 7b represents the word [pu˙ŋoʔo] 'blow (on fire)!', for which CECIL data are:

(7b) Title: 'blow (on fire)!'; Length: 23179 bytes / 1.189secs; Sampling frequency: 19500Hz; Date digitized: 7 December 1990 (+f); Source: CL124.utt; Calc range: 100-350Hz; Voicing threshold: 15; Percent change: 5; Group size: 3

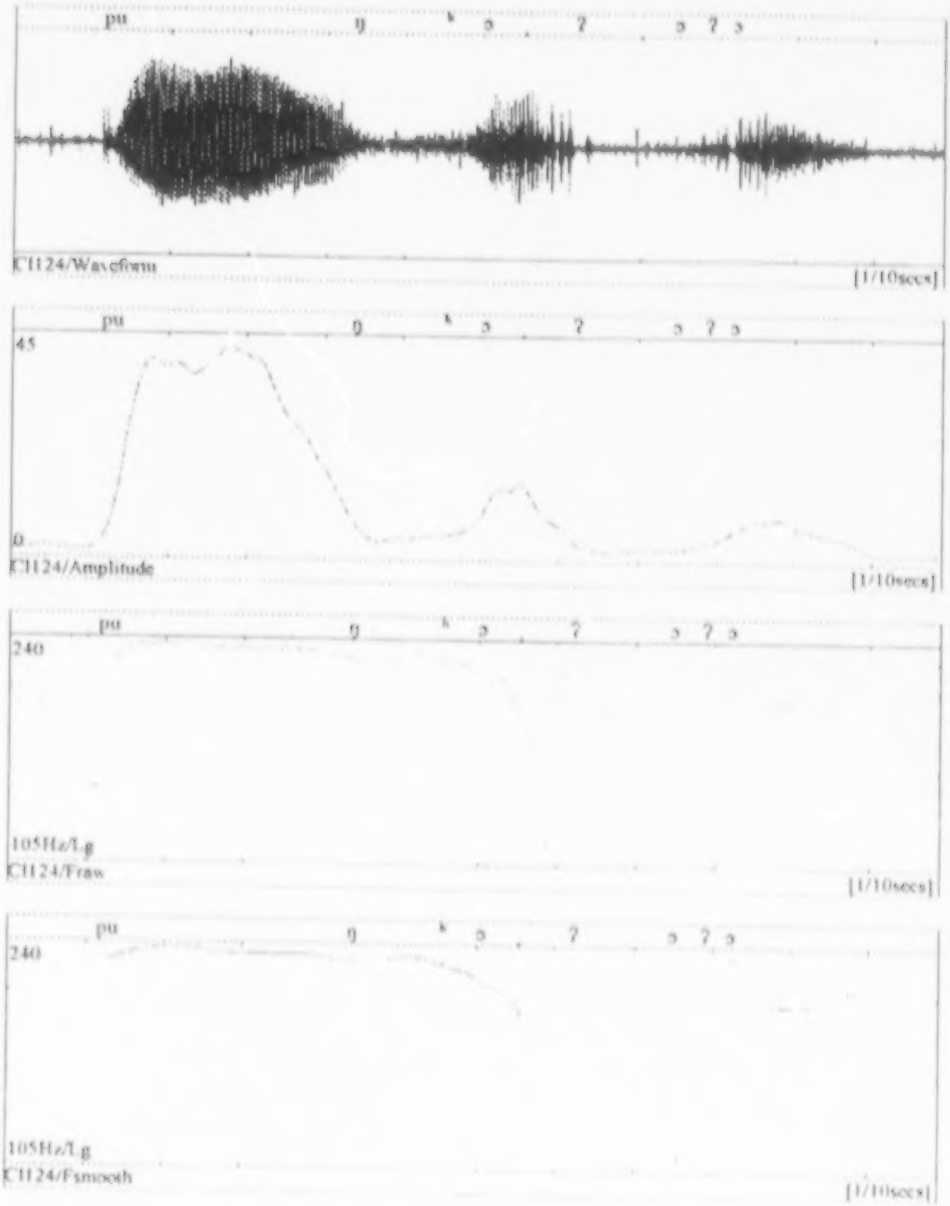


Figure 7b

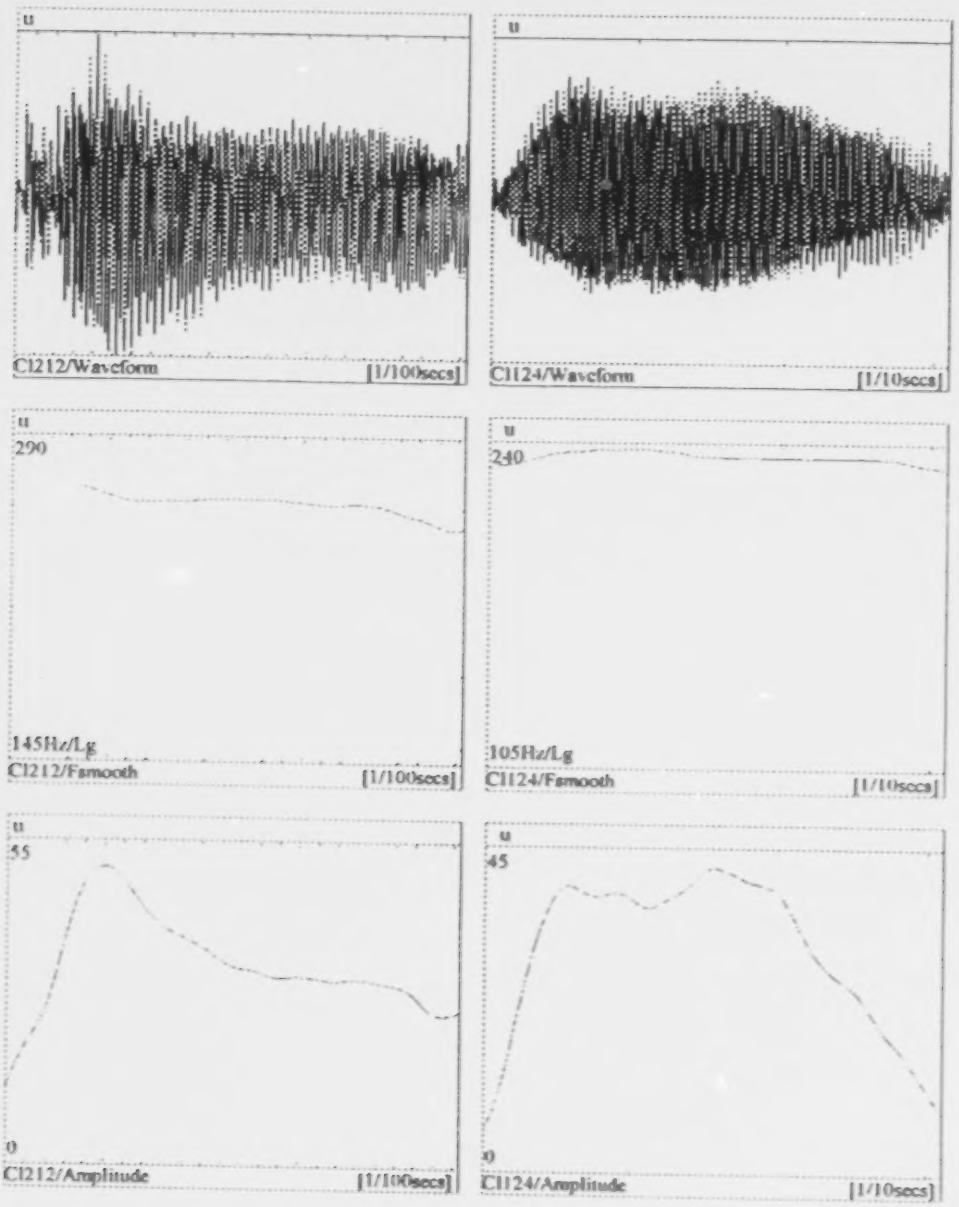


Figure 7c

As in figure 7a the vowel [u·] is monosyllabic and bimoraic, but highest intensity occurs on the second mora. Again pitch holds more or less level until the onset of the velar nasal. Figure 7c offers a close-up comparison of the monosyllabic, bimoraic vowels in each word. Once adjustments are made

for voice pitch range (the speakers are sisters), the only significant difference between the two is the location of the intensity high.[7]

To my ear the high back vowel of 'blow (on fire)' sounds higher than the back high vowel of 'soak the corn!', even though absolute pitch on the vowels is the same when pronounced by the same speaker. The difference must be significant: speakers become annoyed when I cannot reproduce it, even if I pronounce all the other segments and overall pitch patterns correctly. Analogous minimal pairs are beginning to make their appearance as I continue to collect data. Elsewhere in my data a shift in intensity can be related to a shift in the intonational stress (pitch-accent) nucleus (primarily signaled by high pitch). In a minority of words of pitch pattern MHL alternate pronunciations occur where the first two syllables have the same absolute pitch, but the highest intensity hump occurs on the second syllable, instead of the first. These observations have led me to conclude that the shift in intensity signals a lexical tone difference between the bimoraic vowels.

In both of these words the glottal stop is notoriously hard to locate, unlike in figure 6d where the onset (before the end of the vowel, with a typical glottalized echo) is clear, and the release occurs at the beginning of the following vowel. In both 7a and 7b, the vowels preceding the glottal stop closure are glottalized throughout most of its duration. Because both words terminate in glottal stop (unlike figure 6d), the last vowel is glottalized throughout its entire duration.

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[7] I have digitizations of these two words pronounced by the same speaker in which the pitch traces on the bimoraic vowel are almost identical. Unfortunately, no pitch trace appeared for the last syllable in 'soak the corn!', so I substituted the sister's recording here. Elsewhere in my data the word final glottal can be shown to be independent of the positioning of the intensity hump on the bimoraic vowel. Given the intervening syllable, it would be odd if there was a relationship. The velar nasal is sometimes partially devoiced, or more of an approximant, as in figure 7b. This is a free variant which elsewhere in my data can be shown to be independent of the positioning of the intensity hump on analogous monosyllabic, bimoraic vowels.

Figures 8a,b,c: Pitch and stress patterns on disyllabic vowels. The figures in 8 illustrate pitch and stress patterns on disyllabic long vowels. Figure 8c presents a contrast in a related language between a monosyllabic diphthong and a disyllabic vowel cluster analogous to the contrast between monosyllabic and disyllabic long vowels in Chimila..

CECIL data for these figures are:

(8a) Left side: Title: 'maraca'; Length: 6439 bytes / 0.584secs; Sampling frequency: 11025Hz; Date digitized: 4 January 2000 (+f); Source: CL743.utt; Calc range: 40-500Hz; Voicing threshold: 20; Percent change: 10; Group size: 6

Right side: Title: 'heart'; Length: 9006 bytes / 0.817secs; Sampling frequency: 11025Hz; Date digitized: 4 January 2000 (+f); Source: CL744.utt; Calc range: 40-500Hz; Voicing threshold: 20; Percent change: 10; Group size: 6

(8b) Title: 'gourd'; Length: 13432 bytes / 0.609secs; Sampling frequency: 22050Hz; Date digitized: 19 August 1998 (+f); Source: CL661a.utt; Calc range: 40-500Hz; Voicing threshold: 20; Percent change: 10; Group size: 6

(8c) Left side: Title: 'oscuro (dark)'; Length: 14570 bytes / 0.747secs; Sampling frequency: 19500Hz; Date digitized: 14 November 1990 (+f); Source: m1.utt; Calc range: 220-310Hz; Voicing threshold: 13; Percent change: 5; Group size: 3

Right side: Title: 'to see'; Length: 11114 bytes / 0.570secs; Sampling frequency: 19500Hz; Date digitized: 14 November 1990 (+f); Source: m2.utt; Calc range: 170-330Hz; Voicing threshold: 18; Percent change: 5; Group size: 3

Figures 8a,b illustrate a three-way tone contrast on disyllabic long vowels. In figure 8a [too?] 'maraca (rattle)' with pitch pattern HL and highest intensity on the first vowel is compared with [too] 'heart', with pitch pattern LH.[8] Figure 8b shows [too?] 'gourd' with pitch pattern HL and nearly equal intensity on both vowels.

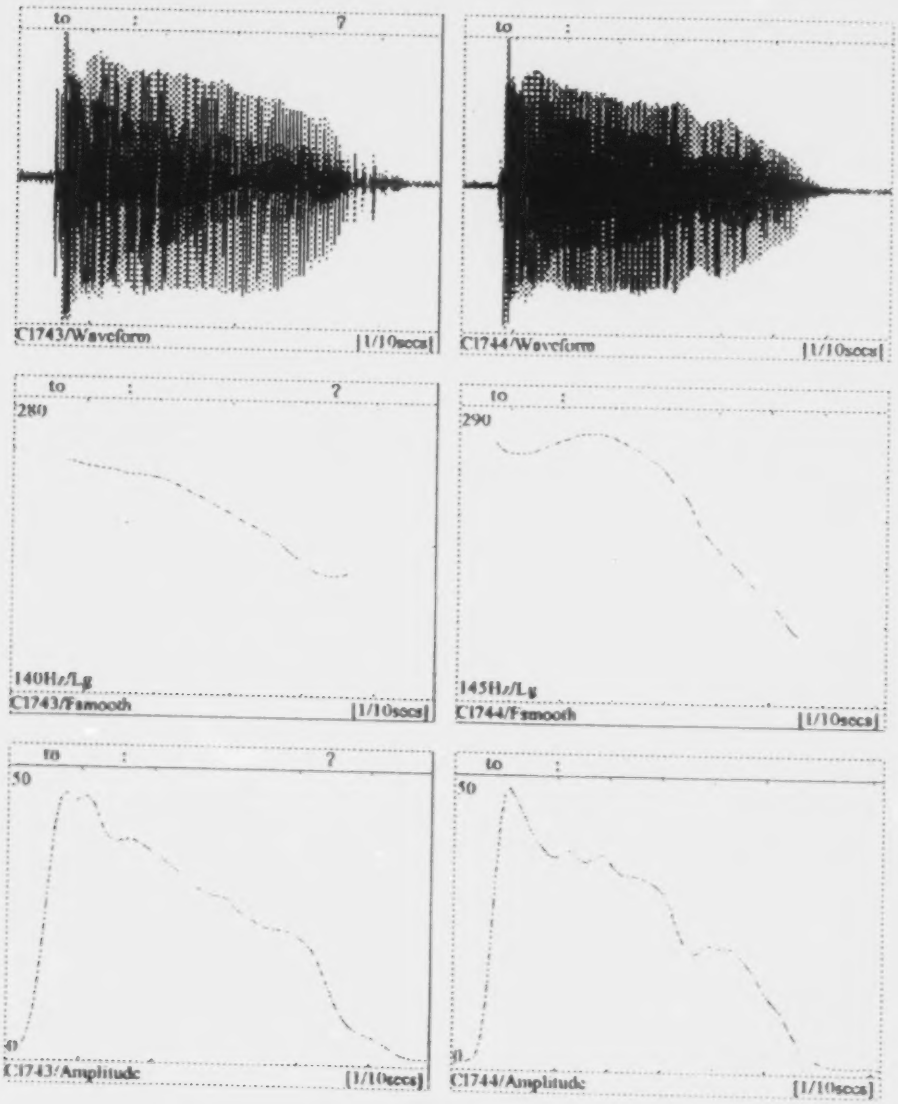

Figure 8a

[8] Some speakers pronounce 'heart' as [too?]. The pitch and intensity patterns are the same as in figure 8b.

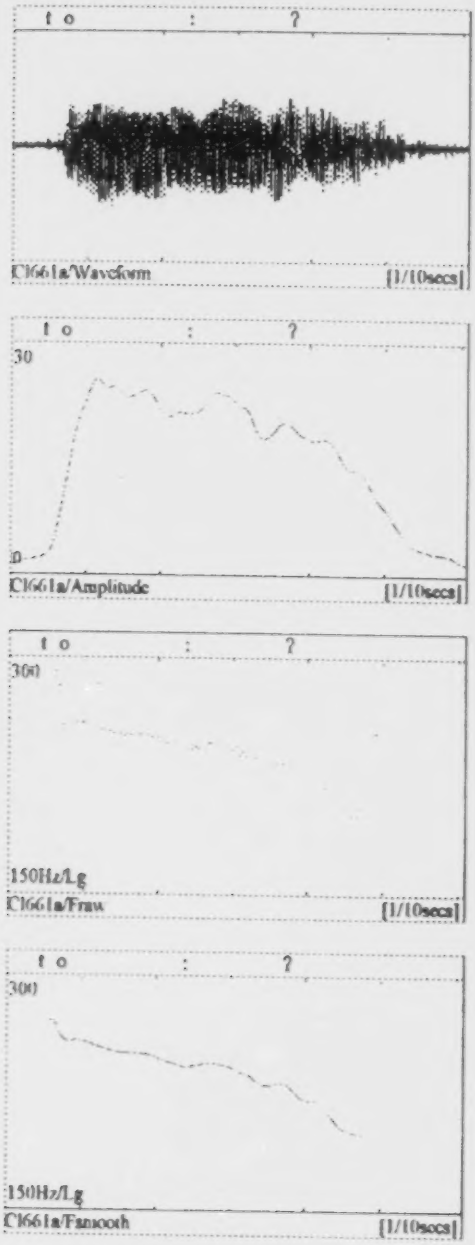

Figure 8b

In these figures I have tried to locate the length symbol ':' at the boundary between the two syllables. The bimoraic vowels have been lengthened an extra mora, due to the words being pronounced in isolation (i.e., the words function as phonological phrases). I have analyzed 'maraca' as unspecified for tone, 'heart' with lexical high tone on the second vowel, and 'gourd' with lexical high tone on the first vowel.[9]

In words that contain disyllabic bimoraic vowels the amplitude trace often reveals the existence of two syllable nuclei. Two intensity humps are present. The humps are not so clear in figures 8a,b. Nevertheless, vowel length, gliding pitch and the behavior of the bimoraic vowels when the roots are suffixed indicate that these vowels must be bimoraic. Curiously, the double humps are most clearly reflected in the waveform figure, especially the darkest section, which narrows at the syllable boundary. The intensity humps are not usually as pronounced for monosyllabic bimoraic or monomoraic vowels. The waveform figures do not usually show the narrowing typical of bisyllabic long vowels. (Compare with figures 7a,b,c.)

When other factors do not intervene the presence of two intensity humps can be used to determine whether a vowel is monosyllabic or disyllabic.

[9] 'Gourd' could conceivably have lexical high tone on both vowels. I have not been able to elicit the word in a context which would eliminate intonational interference.

Figure 8c illustrates a minimal pair for the language Damana (Chibchan, data from Larry and Cindy Williams).

The two vowels in the word 'dark' (left-hand side) are members of separate syllables; the same two vowels in the word for 'to see' (right-hand side) form a monosyllabic diphthong. Other factors tend to intervene in Chimila, with the result that multiple diagnostic factors must be used in order to identify disyllabic bimoraic vowels.

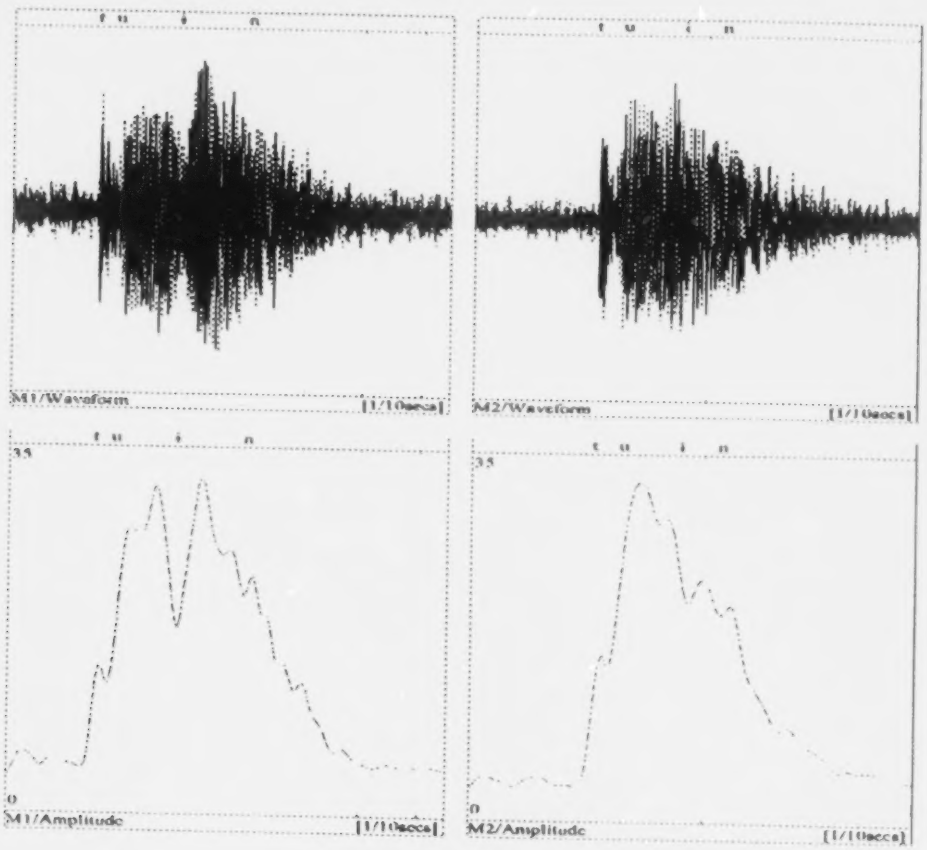

Figure 8c

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REVIEW ARTICLE

The inheritance and innateness of grammars. By MYRNA GOPNIK, ed. 1997. Vancouver Studies in Cognitive Science, Volume 6. NY: Oxford University Press. 232 pp. Cloth $60, paper $29.95 paper.

Reviewed by DONALD A. BURQUEST
University of Texas at Arlington and SIL International

This volume of papers grew out of a 1993 conference on the Biological Foundations of Language, held at Simon Fraser University. The overriding goal of linguistic theory in the generative tradition is to account for language acquisition, and in pursuing this goal generative linguists have proposed that to be human is to be endowed with an innate predisposition to acquire language. This collection of papers attempts to bring experimental research to bear in the evaluation of this proposal, which is to a very large extent supported. It should be of particular interest to *NOLx* readers that data from several different languages are incorporated into the research, and there is a common theme that more such cross-linguistic data relating to the acquisition of language by young children are needed. Because much of the information in this volume is likely to be unfamiliar to field linguists, I have included more detail than is commonly found in a *NOLx* review, summarizing each of the articles in this anthology:

Patricia K. Kuhl and Andrew N. Meltzoff, 'Evolution, nativism and learning in the development of language and speech'.

'Infants acquire language like clockwork' (7), irrespective of the specific language being acquired, the educational background of the parents, or parental prompting, apparently making use of some innate capability. But there remains the important question of how one particular language is acquired, and why its acquisition almost invariably leads to a 'foreign accent' when additional languages are learned. The Native Language Magnet theory of speech development is proposed, consisting of three phases:

Phase 1: Infants are born with innate boundaries that partition the incoming speech stream into phonetically relevant categories.

Phase 2: Exposure to ambient language results in stored memories of the sound patterns that reflect the distributional properties of the infant's native language.

Phase 3: Perceptual space is reconfigured such that certain innate perceptual boundaries have been functionally erased.

In support for Phase 1, the authors cite research demonstrating the ability of young infants to discriminate between phonetic distinctions such as [i] and [a], irrespective of the language(s) to which they will ultimately be exposed. Infants as young as two months generalize the discrimination of phonetic categories across talkers of varying age and of both genders. Experiments indicate that chinchillas and monkeys (species that have auditory systems very similar to humans') have this same ability to discriminate. Such findings argue against any claim that the innate categorical perception effects are evidence of a speech module because animals lack such a module. Rather, the conclusion drawn is that this ability is part of a 'general auditory processing mechanism' (28) common to humans and other animals and is not specific to language. The ability was, however, exploited in the evolutionary development of language, perhaps determining the sorts of sounds useful for phonological contrasts.

In support of Phases 2 and 3 the authors cite studies revealing that infants as young as six months (prior to the time they have uttered or understood their first words) have lost the ability to discriminate between sounds that are not used contrastively in the language being acquired. This is attributed to the 'magnet' effect, in which prototypes of contrastive sounds alter the perceptual space allotted to phonetic distinctions earlier recognized. At this point humans and animals diverge, with animals showing no such magnet effect.

Language acquisition, then, is the result of the interaction between language input and innate capacity. Some of the innate capacity is unique to humans, and associated specifically with language.

Laura Ann Petitto, 'In the beginning: On the genetic and environmental factors that make early language acquisition possible'.

This paper compares the acquisition of signed and spoken language by children. Investigation of children learning either a spoken or a signed language (the languages investigated were spoken English and American Sign Language, and spoken French and *Langue des Signes Québécoise*) reveals that whichever language the child is learning, each child goes through all stages from babbling to full fluency and in a comparable time sequence. Further, for children learning both a spoken and a signed language, they follow the same stages for both modalities at the same time. Finally, hearing children raised in a home with non-hearing parents also exhibit acquisition of the signed language at the same rate and sequence. It

may be concluded, then, that there is no necessary connection between sound and language acquisition, but that children are innately endowed with a general perceptual mechanism that allows for discrimination to take place (perhaps correlating with Phase 1 in the paper above); this is particularly striking because 'the brain-based neural substrates' (51) are different for both the motor control of speech and sign, and for the perception of speech and sign. Children are innately endowed also with a 'structure-recognition mechanism' that allows for distributional patterns to be recognized and acquired (perhaps correlating with Phase 2 in the paper above); this capability allows the child to lay down in memory crucial aspects of the data and establish a motor production loop making use of that information.

Martha B. Crago, Shanley E. M. Allen, and Wendy P. Hough-Eyamie, 'Exploring innateness through cultural and linguistic variation'.

Three studies investigated language acquisition among the Inuit of northern Quebec. The language (Inuktitut) is highly polysynthetic, with words containing six or more affixes not uncommon.

In the first study two sorts of acquisitional environments were identified, those in which the children have young mothers or are cared for by older siblings, and those in which the children are raised by mothers forty years old or older. While neither of the two contexts manifests precisely the same characteristics of the linguistic behavior of care givers found in other cultures that have been investigated, they contrast dramatically when compared with one another. The younger care givers frequently engaged in extensive repetition routines (a characteristic commonly found in other studies as well), while the older mothers not only failed to do that but in addition only rarely labeled objects in the environment, rarely expanded upon children's utterances, and asked the children virtually no questions to invite their response. Indeed, children raised by older mothers were allowed to eavesdrop on adult conversation, but they were deliberately excluded from participation. The fact that Inuktitut-learning children reached the same linguistic milestones, and at the same time as children acquiring non-polysynthetic languages, argues for the existence of 'fundamental innate characteristics of the human mind involved in the acquisition of language' (87) that requires a core of environmental support but not necessarily extensive social interaction.

A second study investigated the stages of acquisition. The one-word stage (treated as the one-morpheme stage for polysynthetic languages) was reached between 0;9 and 1;2; utterances were mostly nouns referring to animate beings, food, and the enactment of personal desires. The two-word (-morpheme) stage was reached between 1;6 and 2;0; for those utterances in

which the two morphemes belonged to the same word, 63% were inflected (as opposed to morpheme combinations which in Inuktitut are one word but phrases in a language like English), quite in contrast to studies of English-learning children, who at the same age use virtually no inflection. Expansion of utterances beyond the two-word (-morpheme) stage followed the same rapid development as that found in other languages, with children younger than 3;0 using word forms and constructions of considerable complexity. Particularly revealing was the use of passives, with 2.8 instances per hour for Inuktitut-learning children 2;0-3;6, compared to 0.4 per hour for English-learning children. Passives are common in Inuktitut, and the forms are simpler (there is no object agreement as in related active forms), but their use by Inuktitut-learning children argues against the hypothesis that acquisition of such forms is necessarily delayed because the subject vs. object contrast and the notion of constituent movement are maturational.

A third study investigated the speech of a five-year-old girl who was deemed normal cognitively and emotionally, but who had impaired speech. Although she had a restricted vocabulary as well, more striking were her deficits in grammar. She manifested no instances of passives; she failed to synthesize forms correctly and tended to use separate morphemes instead (approximating the speech used by Inuktitut speakers when speaking to outsiders as a means of increasing comprehension); she used on verbs affixes normally found only on nominals. The fact that she was cognitively normal argues for an underlying deficit peculiar to language, separating it from cognition generally (compare with the findings of four studies of Specific Language Impairment below).

Four of the papers in this volume investigated the syndrome referred to as Specific Language Impairment (SLI). In general, in this syndrome 'the child has poor language achievement despite normal non-verbal IQ (above an IQ of 85), normal hearing, normal social development, normal emotional status and normal motor skills' (92). That language alone is impaired suggests the existence of a language-specific capability separate from general cognitive ability, but the fourth paper in this series argues to the contrary that SLI is in fact accompanied by impairment in general cognitive ability.

J. Bruce Tomblin, 'Epidemiology of Specific Language Impairment'.

In epidemiology, the rate of occurrence of disease is studied in at attempt to determine causality, although because the technique of observation is used rather than experimentation, such research suggests possible causes, rather than definitive ones. That is the model of research pursued here.

Language-learning problems in SLI subjects appear at the outset of language development and continue throughout the school-age years and often into adulthood. Studies indicate that in the general English-speaking population a rate of 2-3% is found, often accompanied by reading problems. It is striking, however, that there are twice as many instances of males affected as females (a finding not substantiated in subjects in Gopnik, et al., below, cf. 126), and in families with one SLI individual, the likelihood of finding a second such individual is 22%, seven times the rate in the general population. Interestingly, there is no evidence that events during pregnancy, premature birth, obstetrical medication, or home and family characteristics have any relation to the occurrence of SLI. Although detailed twin studies are called for, pilot studies among monozygotic twins do not contradict the hypothesis of a genetic relationship, although the exact model of genetic transmission remains to be determined.

Myrna Gopnik, Jenny Dalalakis, Suzy E. Fukuda, and Shinji Fukuda, 'Familial language impairment'.

This paper considers SLI in three languages, English, Japanese, and Greek, focusing on inflectional morphology in SLI subjects. Taking English past tense verb forms as an example, tests reveal that SLI speakers know the semantic notion [+past] (e.g., they are able to use temporal adverbs referring to past time), but they do not use the notion syntactically. In particular, in spontaneous speech past tense forms are not used, with the unmarked stem used instead; in grammaticality tests the use of the unmarked stem in contexts in which the past tense is required is not recognized as being ungrammatical; in production tests the past tense forms for nonsense stems cannot be produced correctly in accordance with the model that is presented. Agreement patterns also are not used correctly. Two facts regarding text structure are perhaps most interesting: present and past tense forms are used in the same narrative where consistent use of one would be expected, and on the interpretive side, use of full NPs makes the text more easily interpreted than the more expected pattern of pronominalization for continuing reference after first mention. (The fact that SLI subjects show deficits in text structure as well as sentential constructions might be taken to support the argument that the two types of structure are more closely related than proposed in some linguistic theories.) Comparable results are found also in Japanese (especially with regard to tense distribution within text) and Greek (especially with regard to agreement). The authors conclude that SLI speakers know the various inflectional forms of constituents, but they know them as units, not as exhibiting morphological structure. They conclude that SLI speakers lack the ability to construct automatic implicit rules on the basis of primary linguistic input, which they attribute to a genetic deficit.

What progress SLI subjects show with increasing experience (some 20-50% of those diagnosed as preschoolers eventually acquire normal grammars) the authors claim results from memorization, and the conscious implementation of pedagogical grammatical rules such as those learned in school. In contrast to Tomblin's conclusions regarding a preponderance of SLI subjects' being male, these authors find that the existence of SLI deficit is virtually equally common in both genders among their subjects.

Harald Clahsen and Detlef Hansen, 'The grammatical agreement deficit in Specific Language Impairment: Evidence from therapy experiments'.

This paper is closely related to the preceding. The authors summarize three hypotheses proposed to account for SLI: 1) difficulty in acquiring grammatical morphemes with low phonetic substance, 2) difficulty in gaining access to regular rules of inflection, 3) difficulty in establishing agreement relations between two elements in phrase structure. Using the model of Generalized Phrase Structure Grammar, this paper argues that the deficit lies in difficulty with agreement. SLI subjects acquiring German typically use zero inflection or infinitives, with delayed acquisition of inflected forms, but they do form participles by affixation and in fact overgeneralize the unmarked form, showing an ability to use morphology. They manifest correct word order within NP, PP, AP, but they typically have verb-final constructions in main clauses, with the verbs in non-finite form, although they use some auxiliaries and modals correctly and distribute them in second position (referred to as the verb-second (V2) pattern) as required for inflected forms. Final position is the correct position for non-finite verb forms, but in main clauses without auxiliaries these verbs should not be non-finite and should occur in V2. Nonimpaired children show the same pattern, but once inflection is acquired (at about 2;5) they correctly distribute such verbs in second position.

Four subjects were identified for treatment, with an average age of 6;5. Therapy was designed to teach children agreement without direct evidence for the V2 position for verbs. Children were given 90 therapy sessions each over 14 months, with much repetition of the same verb in its various forms and in a form of interaction with the therapist appropriate for children. The children acquired the inflected forms in the same order as that found in nonimpaired children, and there was a comparable development also in positioning the forms in V2 in spontaneous speech. The authors conclude that it is a deficit in grammatical agreement that is the problem in SLI subjects, with the deficit in V2 positioning following as a secondary effect. It is explicitly argued that the deficit is not one of phonetic salience because

the same form *-n* is used correctly in participle formation but incorrectly in inflection. It is explicitly argued also that the deficit is not one of lack of morphological capability because the participles are formed, and in fact with an overgeneralization of the unmarked *-t* suffix manifesting knowledge of a rule.

Judith R. Johnston, 'Specific Language Impairment, cognition and the biological basis of language'.

In contrast to the preceding papers, Johnston argues that studies indicate a general cognitive deficit in SLI subjects. In non-verbal reasoning problems requiring higher-level conceptual problem-solving, SLI subjects are only 23% successful, compared with the 65% rate of success found in nonimpaired subjects. Still, it can be argued that perhaps the language deficit impedes cognitive performance because language provides a means of representing interpretations of the world, summarizing thought, and allowing use of precompiled mental routines.

This paper investigates a second question, whether SLI subjects manifest cognitive deficits that cannot be attributed to language (and therefore suggesting an account for the delay seen in SLI subjects). Providing evidence based upon studies dealing with non-verbal behavior, it is argued that such a connection can be established:

1) In terms of EARLY SYMBOLIC PLAY, SLI subjects engage in less, or less sophisticated, play—perhaps indicating some special problem with symbolic functions.

2) Confronted with two BRIEF AUDITORY STIMULI in sequence, SLI subjects matched nonimpaired subjects in terms of identifying whether one or two stimuli occurred, but they required an interval between stimuli ten times longer than that required for nonimpaired subjects before they could accurately establish the order of occurrence.

3) In VISUAL IMAGERY tests making similarity judgments of sequences of geometric forms, if one array of forms is rotated with reference to the alignment of the other, both SLI subjects and nonimpaired subjects take longer to solve the problem the more radical the rotation, but SLI subjects take longer than nonimpaired subjects. It is argued that all subjects make use of non-verbal strategies to solve the problem because if language were being relied on there would be no difference in the effect of degree of rotation on processing time. The fact that SLI subjects still require a greater processing time than nonimpaired subjects argues that the deficit found in SLI subjects must be non-verbal.

4) SLI subjects also show a deficit in ATTENTIONAL CAPACITY. Children were given a non-verbal matching task to perform, which could be interrupted by a second, more elementary task (press a button to turn off a buzzer). SLI subjects acted more slowly to turn off the buzzer, suggesting that their attentional resources were more restricted and were being largely occupied by the primary task.

The author concludes that such information-processing deficits (especially in perception, processing rate, and attentional capacity) can account for the facts of delayed language acquisition manifested by SLI subjects. It is argued that 'Specific Language Impairment' is perhaps not so 'specific' after all.

Steven Pinker, 'Evolutionary biology and the evolution of language' (adapted from his 1994 book, *The Language Instinct*).

This paper makes important claims and is the most entertaining chapter of the book. Pinker makes forceful arguments in support of his position, but he does so with a refreshing sense of humor and creative use of language (a writing style more linguists could make use of).

Pinker asserts that language is unique to human beings, and he presents a useful summary of the theories of evolution and natural selection in supporting his proposal regarding how language ability could have developed. The well known experiments with great apes do not count as counterevidence; their language ability remains qualitatively distinct from that of humans. Nor should it be expected that there be found in chimpanzees (the closest genetic relatives to humans) anything approximating language ability even in a rudimentary form. With the evolutionary claim of separation of the ancestors of humans and the ancestors of modern apes 5-7 million years ago (350,000 generations), there is no reason to expect that the development of the language ability leading to the full-blown Universal Grammar seen today could not have begun after that separation and evolved gradually by widely accepted principles of evolution. Nor is the claim that humans and chimpanzees share 99% of DNA significant--on the one hand the 1% difference allows plenty of room for Universal Grammar; and in any case, it must be kept in mind that a 1% difference in DNA could well be generally distributed, resulting in a 100% difference in gene makeup if each gene differs by just 1% in its DNA.

Three questions particularly are addressed:

1) Can new modules such as language evolve (a claim contrary to that of Lieberman)? Yes.

2) Did language evolve by natural selection (a claim contrary to that of Chomsky, Gould, and Lewontin)? Yes.

3) Could language ability have evolved gradually (a claim contrary to that of numerous scholars in the field)? Yes.

As a parting shot Pinker points out that 'anthropologists have noted that tribal chiefs are often both gifted orators and highly polygynous' (204), a pairing of facts that might help account for how linguistic skills, once developed and proven to be highly important in fostering the intricacies of culture, can easily become widespread.

Harvey M. Sussman, 'A neurobiological approach to the noninvariance problem in stop consonant categorization'.

Because the notion of syntactic structure proposed for Universal Grammar is abstract, it cannot be used to establish a principled link between brain and language. This paper investigates whether the notion of phonemic contrast can do so and provides a detailed neurological proposal to demonstrate how this might be accomplished.

It is well known that speakers ignore allophonic variation in identifying stop consonants. Because of the 'strong degree of similarity of the brain, in both structure and function, across species' (210), perhaps a link can be established between documented animal processing algorithms and speculative human processing algorithms. Barn owls are able to identify the location of objects by evaluating the relation between the input received by each of their two ears. Research shows that the owl brain compares the phase of the acoustic signal entering each ear and by evaluating the percentage of difference in the phases of the two signals is able to compute the angle of the object to one side or the other. Crucially, because the percentage of the phase difference varies with the frequency of the signal (correlating inversely with the wave length), the owl brain must ignore that variation and extrapolate to a generalization valid for all frequencies. In a comparable way, the mustached bat is able to establish the velocity and distance of an object on the basis of the Doppler shift effects on a combination of different frequencies. Could it be that the human brain functions in a similar manner to determine phonological contrasts?

Given a CV syllable, plotting the onset frequency of F2 transitions from C to V against the offset frequency measured at the midvowel target nucleus results in a slope on a graphic display for which a locus equation can be written. Experimentation with twenty speakers of different dialects of American English pronouncing /b/, /d/, and /g/ with a variety of following vowels reveals that almost 90% of the tokens lie on the slope defined by the

locus equation (comparable facts are found in research with speakers of Thai, Cairene Arabic, and Urdu). Each stop has its own characteristic transition to the F2 of the vowel with which it occurs, so that although the vowel (as well as speaker and dialect) varies, the relationships among the three stops remain constant (there is some overlap with specific vowels, and it is proposed that perhaps incorporating information regarding F3 onsets can be used to make the final differentiation in at least some contexts).

Just as the barn owl and mustached bat evaluate the relationship between two acoustic facts, so does the human brain. The result is 'abstracted phonological equivalents' (227) for each CV combination. The neurons used by the mustached bat are apparently exactly the type needed for the neural representation of contrastive sound categories in the human auditory system. Assuming that speech perception processes one CV at a time, the question remains as to how the relational nature required for the interpretation of the different consonants is arrived at. A detailed neural account is proposed which bears a striking resemblance to the AND and OR switches used in Lamb's Stratificational Grammar.

This is an important and thought-provoking volume of papers for linguists who are looking for experimental data in relation to the claim of innateness. An underlying theme throughout is the value of cross-linguistic studies, particularly studies investigating interaction between care givers and young children. This is an area of research which many readers of *NOLx*, with perhaps some specific additional training or in cooperation with specialists, are particularly well positioned to carry out. The papers included here provide a good point of reference for claims in the field and for insights as to how such research can be conducted. The book is well produced, with very few typographical errors (none hindering understanding). Although somewhat esoteric for the interests of the field linguist this book can be a worthy addition to the bookshelf.

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REVIEWS

Creole genesis and the acquisition of grammar: The case of Haitian creole. By CLAIRE LEFEBVRE. 1998. Cambridge Studies in Linguistics. Cambridge: Cambridge University Press. 461 pp. Hardback $74.95.

Reviewed by GEORGE HUTTAR
SIL—Africa Area

What does this book, of major importance for creolists, have of interest for the majority of *NOLx* readers? First, it is a model of careful argumentation, including clear description of the methods and assumptions involved in the underlying research. It is explicit, for example, about its hypotheses, and about what kinds of data would support the hypotheses, or disconfirm them, or be irrelevant and therefore ignorable. For that alone I commend the book to your attention. Second, virtually every language readers are focusing their fieldwork on today has been affected by present—and usually also past—contact with other languages, resulting in sometimes puzzling dissimilarities with closely related and neighboring languages. In its documentation of the way one language, Haitian Creole, reflects influence from different languages in different parts of the language—lexicon, phonology, specific parts of morphosyntax—this book may shed some light on some of those dissimilarities. Better, though, it can give you some ideas of how to use what's known about related and neighboring languages to come up with hypotheses about particular subsystems of the language of your interest. The processes at work also shed some light on the kinds of changes you can expect speakers of language X to make in a national language as they acquire it, and mistakes you can be on guard against yourself as you try to master a vernacular or a national language, with your own native language competence ever lurking in your mental background.

But the book is admittedly written for those with special interest in pidgin and creole languages, so the rest of this review provides some background to the controversy on which it takes a position, including its relation to Chomskyan questions of universal grammar and language acquisition, then all too briefly summarizes its methods and conclusions.

Over the last several decades, but especially since Bickerton (1981) first advanced his 'bioprogram hypothesis', students of pidgin and creole languages have debated vigorously the question of where such languages get their phonology and, especially, their morphosyntax. The main issues are easily understood if we use a specific example: Speakers of many different

African languages were brought to Haiti as slaves during the 17th-19th centuries. Eventually a new language, Haitian Creole, emerged. Much of its 'everyday' lexicon is clearly derived from French, but what about its phonology and morphosyntax? Are they basically French (superstrate—i.e., the language of those in power), or African (substrate—the languages of the sociopolitically subordinate)? Or do they reflect the language universals with which any newborn human comes already 'programmed' (Bickerton)? Or are they a result of universal principles of second language acquisition carried out in a particular sociolinguistic context, that of Haitian plantations where the degree and nature of contact between native French speakers and speakers of African languages changed over time? Or has some combination of these and similar factors been at work? In answering such questions, we also need to explain how it is that such languages in different parts of the world, with no clearly demonstrable historical connection, have some similarities in their morphosyntax.

Eventually three basic positions have emerged: The morphosyntax of pidgins and creoles[1] is due to innate language universals, it is due to the morphosyntax of the substrate languages, or it is due to some combination of these. Aspects of second language acquisition are adduced in support of any of these positions (disagreeing principally on whether it is adults or children who create a creole), which all parties also acknowledge that the superstrate language makes a contribution—as it obviously does in the lexicon. Variations on these themes, especially the third position, involve attempts to identify which features of a creole can be traced to substrate sources, which to superstrate, and which to innate universals. It is no wonder that most creolists see their work as contributing in an important way to Chomsky's program of identifying universal principles and parameters of the human language capacity, as well as to L2 acquisition theory.

To seriously investigate the extent to which substrate languages have contributed to the morphosyntax of a particular creole, such as Haitian Creole (HC), detailed study of the superstrate, of the known substrate languages[2], and of the creole itself is required. All of these are to be examined as close to the time of the formation of the creole as we can find information, rather than on any of these languages in their current form. Although the whole exercise increases our understanding both of innate language universals and of L2 acquisition, at the same time what is already

[1] I ignore here the important differences between the two, even with regard to this question of origins.

[2] Substrate languages can be identified partly from lexical items in a creole that are traceable to a particular language, but so far extralinguistic sources, such as slave trade shipping records, have been especially helpful.

known about these two areas helps us evaluate the hypotheses we come up with in examining a particular creole, its substrate and its superstrate.

The author of the book under review, Claire Lefebvre, has taken this empirical challenge seriously, leading for a decade-and-a-half a research team (at the Université du Québec à Montréal) comparing HC, French, and Fongbe, a variety of the Kwa language Gbe known from both external and internal evidence to have been the language of a large proportion of the slaves exported to Haiti during the period of HC's formation. The present book summarizes the methods and findings of the research and synthesizes L's conclusions (394):

> Both historical...and linguistic...evidence points to the conclusion that Haitian creole was created by adult native speakers [who] used the properties of their lexicons and grammars in creating the creole. The division of the properties found in the creole argues that the genesis of creole languages is a particular case of second language acquisition in a context where the substratum speakers have little exposure to the superstratum language. This explains why substratum speakers rely on relexification to create a new lexicon, and on the principles and parametric values of their own grammar to establish the grammatical properties of the new language they are creating....this strategy affects all components of the grammar. Using the properties of one's own lexicon and grammar in order to create a new language rapidly is the most economical way of doing it.... The data and analyses presented here support the claim that pidgin and creole genesis can be accounted for in terms of the basic processes already known to play a role in language change in general: relexification, reanalysis and dialect levelling.

The chief process of the three just named is relexification: Basically, this means that a creole gets the phonological shape of most of its lexemes from the superstrate, but retains the morphosyntactic and semantic features of lexemes from one or more substrate languages.[3] So if HC *bon* 'good' gets its phonological shape from French *bon* 'good', its grammatical and semantic behavior very closely parallels that of lexemes for 'good' in Gbe languages, not that of French *bon*.

L's evidence and argumentation leading to such a conclusion I find convincing (though all do not—there has been heated debate about some points on the CreoLIST email discussion list). As mentioned above, she carefully sets out her hypotheses and discusses what kind of data would support them and what would falsify them. She investigates in detail a wide

[3] 'Reanalysis' has to do with a form signalling a particular lexical entry changing to signal another one, as in grammaticalization, while 'dialect levelling' deals with the fact that a variety of (often similar) substrate languages may be involved in a particular case of creole genesis, with some features of the creole being traceable to other varieties than the one that appears to have contributed the most to the new language.

range of morphosyntactic phenomena: NPs, preverbal TMA markers, pronouns, functional lexical entries (e.g., interrogative, negative, complementizers), determiners, clause structure, syntactic properties of ten or so subclasses of verbs, derivational processes, compounding, and Chomskyan parameters. Some parts are more convincing than others, and occasionally one wonders whether there is too facile an appeal to 'dialect levelling' to account for cases where HC does not match Fongbe any better than it does French. But overall the research and argumentation give solid support for seeing the substrate languages of a creole as being the main source of semantic and morphosyntactic structure. This by no means rules out a role for linguistic universals, but gives much more place for the substrate contribution than strong proponents of the language bioprogram hypothesis have usually been willing to consider. Of special interest is L's finding that the substrate contribution is evident in functional lexical entries, not just content ones.

L's bibliography is impressively thorough, but be on guard for surprising inaccuracies of dates.

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George Huttar, Box 24686, Nairobi, Kenya.
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Spanish Grammar. By JOHN BUTT. Oxford: Oxford University Press. 1996. 350 pp. Paper £4.99.

Reviewed by NEILE A. KIRK[1]
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Spanish Grammar is an excellent little book which I can wholeheartedly recommend for use by beginners or near-beginners with or without access to a teacher. It is arranged into sections: Verbs, Articles, Nouns, Personal pronouns, Indefinite pronouns, Relative pronouns, Adjectives, Demonstrative pronouns and adjectives, Possessives, Neuter pronouns and articles, Adverbs, Negation, Personal a, Prepositions, Conjunctions, Numbers, time and quantities, Questions, 'For n days/weeks', 'ago', 'since' and similar expressions, Affective suffixes, Word order, Pronunciation,

[1] I would like to thank members of the Parkville Circle for useful discussions and advice on the final version of this manuscript.

Spelling and punctuation, Translation traps, and Verb forms. There is also a Glossary of grammatical terms and an Index, enabling the reader to find solutions to particular difficulties with the language.

Butt (B) makes clear that the global distribution of Spanish makes it a hotbed of diatopic variation:

> For example, a ball-point pen is *un bolígrafo* in Spain, *una birome* in Argentina, *un lapicero* in Peru and Central America, *un esfero* in Colombia and *una pluma* or *una pluma atómica* in Mexico, although in this, as in many similar cases, the word used in Spain is understood by many people everywhere. (v.)

A number of Spanish words such as *placer* and *cordillera* has been taken into English. This has often tended to be in places like California whose goldfields were among the many English-Spanish contact areas following the conquest of large areas of Mexico by the United States in 1848.

The section on nouns is excellent and can serve as a useful reference even for people who are well beyond the beginner's stage. Particularly useful is the list of homonyms whose meanings are differentiated by grammatical gender (*el* is the masculine definite article, and *la* the feminine), such as (96):

el capital 'capital (money)'	la capital 'capital (city)'
el cólera 'cholera'	la cólera 'wrath/anger'
el coma 'coma'	la coma 'comma'
el cometa 'comet'	la cometa 'kite (the sort you fly)'

An interesting feature of gender variation is in professional designations, whereby (99):

> Some words denoting professions tend to be invariable in formal language, especially in Spain, but they may form their feminine with *-a* in informal speech, although usage is at present in flux. The formal form is safer and is felt to be more respectful:

MASCULINE	FORMAL/INFORMAL FEMENINE
el jefe 'boss'	la jefe/la jefa
el juez 'judge'	la juez/la jueza
el médico 'doctor'	la médico/la médica

So with these and some other examples which B gives, the Spanish language is changing in response to changes in society—a situation which should have some interest for most linguists.

The presentation of sometimes difficult topics like the verbs *ser* and *estar*, which both correspond to English *to be*, is commendably clear. B invites us to (78):

> Compare *la nieve está negra* 'the snow's black' (because of the soot, dirt) and *la nieve es blanca* 'snow is white' (its natural state), or *eres muy guapa* 'you're very attractive' and *estás muy guapa* 'you're LOOKING very attractive'.

This section is one of the most valuable in the book.

The section on pronunciation is a model of pedagogical clarity. As with the lexicon, there are regional variations in pronunciation which the learner should be aware of. For example, '*j* is pronounced in Spain and most of Argentina like the *ch* in Scottish *loch* (phonetic sign [X]). In most of the rest of Latin America it is soft like the *h* in English *hat*.' (219)

A particularly attractive feature of this book is the inclusion of the section 'Translation Traps', which includes important points which are not always clear to anglophones who learn Spanish. For example, under the heading 'Afternoon, evening' B explains that (230):

> It is difficult to differentiate these words in Spanish, since *la tarde* runs from about 1 p. m. to after sunset and therefore includes our afternoon and evening. *La noche* begins around 8 or 9 p.m.

The 'Glossary of grammatical terms' helps ensure that the grammatical terms used through the text serve rather than hinder the reader's use of *Spanish Grammar*.

Spanish Grammar is a useful volume to have in one's personal collection, and extremely affordable. This book is well worth acquiring for a variety of categories of English-speaking people who are mastering or have mastered the basics of Spanish, which is a language which more linguists should get to know. This will avoid too many recurrences of the situation like that described by Sidwell (1998:142-143):

> The great landmark in historical vascological studies is Luis Michelena's 1957 paper *Las antiguas consonantes vascas* [and] being written in Spanish, a generation of linguists who don't know languages have ignored it.

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Parts and wholes in semantics. By FRIEDERIKE MOLTMANN. New York: Oxford University Press. 1997. 265 pp. Cloth $39.95.

Reviewed by JOOST ZWARTS
SIL—Kenya

There are probably few areas in semantics that enjoy such a lively interest of formal semanticists as the interpretation of plurals, and this results in a steady stream of publications. This book is another contribution to that area, taking part-whole structure as its line of approach. Contrary to what one might tend to expect on the basis of the title, the author does not offer a comprehensive treatment of the role that parts and wholes play in linguistic semantics. The focus is on what Moltmann calls 'situated part structures', basically the part structures that plural nouns (like *members*) and collective nouns (like *family*) have in particular situations and the relevance of these part structures for quantification and the collective/distributive distinction. M distinguishes these situated parts from what she calls 'functional parts', like the surface of an object or the door of a house and this kind of parts are only briefly mentioned.

The basic idea of the book is that an entity may have different part structures in different situations. This sounds like an obvious idea, but it contrasts with the mainstream semantic literature about plurality which assumes that the relation between wholes and parts is fixed in one universe of discourse (model). Instead of this more traditional 'extensional' (set-theoretic and mereological) approaches to plurality, M offers an 'intensional' approach in which the part structure of an object is a function of situations (roughly, small-scale possible worlds). One important notion defined within the resulting framework is that of an 'integrated whole'. For instance, an apple in its usual manifestation is an integrated whole, but there are possible situations in which the apple is cut to pieces. Count nouns typically refer to integrated wholes (as in *The salad contains an apple*), mass nouns refer to entities that lack integrity in their part structure (as in *The salad contains apple*). This way of looking at the mass-count distinction is, as M notes, a bit like that of Langacker (1987), who uses the notion of 'boundary' instead of integration, but she claims to have formalized the notions that he left 'intuitive and informal'.

After having presented the basic idea in Ch. 1, M applies her proposal in Ch. 2 to the analysis of distributivity, arguing that the way a predicate applies to its argument (like in the inevitable sentence *The men lifted the table*, where the men can do their job individually, as a whole group, or in various subgroups) depends on the situation in which the sentence is interpreted and which determines the part structure of the argument. Ch. 3 discusses how

certain predicates select for an argument with a particular kind of part structure, like *enumerate* or *between* (which both need objects with accessible parts). Ch. 4 deals with the interaction between (generalized) quantifiers and part structures. In Ch. 5 M studies some ways in which the part structure of an expression can be specified lexically, by words like *frequent* and various quantifier-like words in German. Ch. 6 shows that part structure does not only depend on the situation, but also on the dimension with respect to which an expression is interpreted. For example, a family may not be an integrated whole in the dimension of space, because the members are scattered across the earth, but in the dimension of kinship they can still be one whole. In Ch. 7 M argues that, contrary to what is commonly assumed, verbs do not exhibit the mass-count distinction (states being mass, events being count, roughly), but that all verbs are mass. There is a small Ch. 8 with concluding remarks and an index.

M's book contains a more varied range of linguistic data than most other formal semantic works about plurality and it offers an original way of approaching these data. Being about semantic phenomena in English and German, the book is of limited interest to field linguists. The model-theoretic framework that is used requires from readers some familiarity with formal semantics for the book to be useful to them.

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Books Available for Review

The following books are available for review by *NOLx* readers. The reviewer keeps the book in exchange for submitting a publishable review. Contact by email <mike_cahill@sil.org> or snailmail: Notes on Linguistics, 7500 W. Camp Wisdom Road, Dallas, TX 75236 USA.

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WORKSHOP ON THE PHONETICS AND PHONOLOGY OF PROSODY, 2001

The analysis of stress, intonation, and tone in natural languages is notoriously difficult. Yet these phenomena and others closely associated with them are vital for a thorough understanding of the grammar of many languages. The difficulties of analysis in these domains are both phonological and phonetic. Phonologically, a proper understanding of prosody requires analysis of many interlocking areas of linguistics. The research needs to know about syllables, moras, morphophonology, syntax, and, quite often, discourse to make headway. Consequently, prosody is frequently underanalyzed and misanalyzed crosslinguistically. More generally, understanding in this area is impeded when there is a lack of systematic analysis of the phonetics underlying the phonology of prosody.

The 'Phonetics and Phonology of Prosody' workshop offered through the University of North Dakota-SIL International Summer 2001 Session will offer the following helps for researchers from any part of the world:

1) Instruction in the acoustic phonetics bases and correlates of intonation, stress, and tone crosslinguistically, along with hands-on learning of PC and MAC tools for speech analysis (especially SIL International's *Speech Analyzer* software and the *PRAAT* program developed by researchers at the University of Amsterdam);
2) Regular individual sessions with participants on analyzing prosodic features of their languages of concern;
3) Presentations from workshop organizers and participants on the phonological and phonetic nature of prosody in different areas of the world;
4) Help and instruction in writing research results for publication. Helped will be offered for writing either descriptive or more theoretically-oriented reports. We hope to edit and publish a volume of research reports from the workshop.

This workshop is available to linguists currently engaged in fieldwork who have a minimum of one course each in phonology and grammar/morphosyntax. No particular theoretical background is required, nor is any previous training in acoustics phonetics. Participants in the workshop must be enrolled as students of the University of North Dakota - SIL summer session. Those linguists who do not need or wish to receive academic credit for their participation will still need to enroll to audit the workshop as a course. (This will enable them to receive special rates for room & board during the workshop.) Participants wishing to receive academic credit for the workshop may enroll for 2-5 graduate credits, depending on the type and level of research they plan to engage in. The workshop will begin during the second week of June and will run until the second week of August, 2001. Exact dates will be provided in a later announcement.

For further information, contact dan_everett@sil.org or visit the website of the University of North Dakota – SIL summer session: http://www.nd.sil.org

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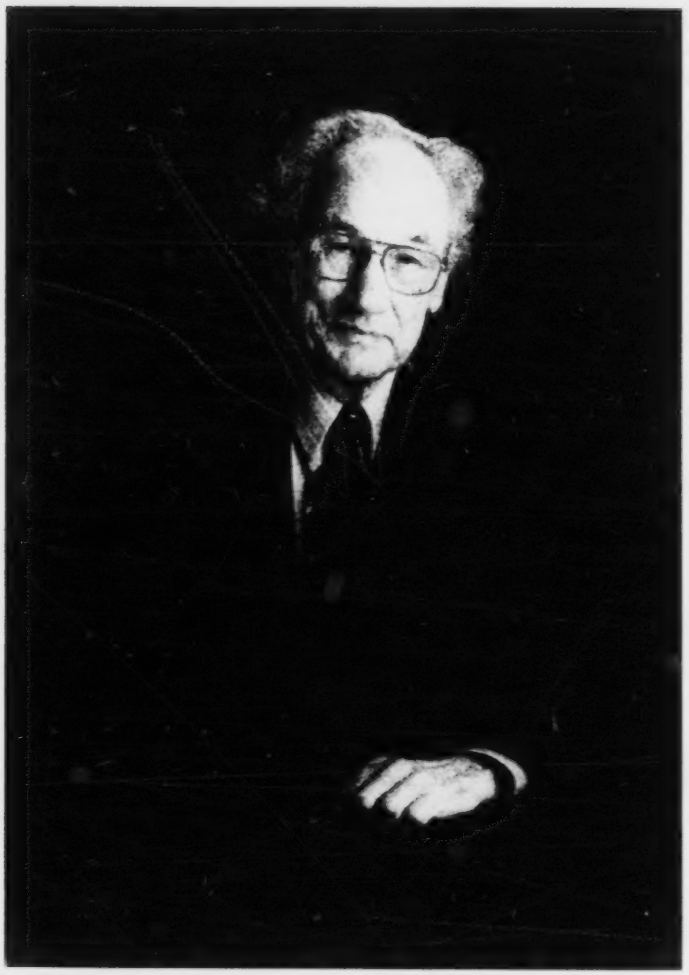

In Memory of Kenneth L. Pike

SIL International President Emeritus Kenneth Lee Pike died Sunday, December 31, 2001, at 7:15 PM in Dallas, Texas. Dr. Pike became ill on Christmas evening and was admitted to Charlton-Methodist Hospital on the evening of December 30th.

Dr. Pike was born in East Woodstock, Connecticut on June 9, 1912, the next to youngest of eight children of a country doctor, Ernest R. Pike. He attended Gordon College of Theology and Missions (then in Boston). In 1937 he joined the Summer Institute of Linguistics and served in Mexico working on the Mixtec language. He later attended the University of Michigan where he received his Ph.D. in Linguistics under Charles Fries and later served for thirty years on the faculty. Pike was the recipient of numerous honorary degrees from universities around the world, President of the Linguistic Society of America, the Linguistic Associa-

tion of Canada and the U.S., and a member of the National Academy of Sciences of the United States of America. He was nominated for the Nobel Peace Prize.

Dr. Pike led a distinguished career as a linguist and Christian statesman, and touched the lives of many through more than sixty years of service. One of Pike's major goals was to help colleagues with their linguistic challenges. To that end, he established workshops around the world, helped thousands of students and field researchers with thorny analytical issues in minority languages, and contributed greatly to the social sciences through his notion of the etic/emic distinction. He was actively involved in academic publication until his death.

He became President of SIL in 1942 and continued in that role until 1979. Pike divided his time between the University of Michigan and SIL, as Director of SIL at the University of Oklahoma and helped establish SIL schools around the world. A private memorial service for the family members was held on January 5th. A public memorial service was held at the First Baptist Church of Duncanville on January 6th. Memorial gifts may be made to the Pike Scholarship Fund at SIL International, 7500 W. Camp Wisdom Road, Dallas, Texas, 75236 U.S.A.

■

An annotated bibliography of basic acoustic theory for the field linguist

Terry Malone
SIL—Colombia

[**Editor's note**: This article is the last in a series of three articles by the author discussing the use of CECIL, a computer program or system used for the acoustic analysis of speech. The first article 'Is CECIL worth the bother?' is in *Notes on Linguistics* 3.1. The second, 'Interpreting CECIL frames: Examples from Chimila' is in *Notes on Linguistics* 3.2.]

In his article 'What to do with CECIL?' Joan Baart comments that field linguists using CECIL need '...a systematic overview of the findings of acoustic phoneticians in a number of areas, showing them what to expect and look out for' (1996:18). When I first started using CECIL, I didn't realize this until I had digitized two or three hundred words and phrases. At that point I started reading books and applying what I could find that was of use to CECIL diagrams. The result was a fairly comprehensive general introduction to acoustic phonetics, as well as enough information to interpret diagrams for Chimila and come up with the analysis partially described in two previous articles in this journal.

The books listed and annotated below are not the final answer to the situation that Baart discusses. Even his well-written introduction does not entirely replace a good technical library, including series of major journals currently published on acoustic linguistics. This is where field linguists will have to go in order to deal with special problems that are bound to turn up as they deal with previously undescribed languages. [1]

I recommend that all the books in this bibliography be in a branch library where a significant number of field linguists are using CECIL. The field linguist who has had little exposure to acoustic phonetics but who wants to use CECIL would do well to begin with Baart 1999, supplement this work with Fry 1979 or Ladefoged 1996, and make sure that Laver 1994 and Ladefoged and Maddieson 1996 are at hand.

The neophyte (and the veteran) CECIL user should be aware that there are major theoretical differences between some of the authors in this bibliography, both with respect to phonetic and phonological theory. The Atlantic Ocean seems to be the major dividing point, though practitioners

[1] Fortunately, SIL field linguists have available the excellent service offered by Wycliffe Associates UK. They are willing to locate and send copies of technical articles to all four corners of the earth. Contact: <Wyc_Associates_UK@wycliife.org>.

can be found on both sides who are exceptions. Of the two major theoretical persuasions, practitioners of various American theories of Generative Phonology seem to be less conscious (judging by the works reviewed here) of the theoretical divide, although this is changing due to the recent development of 'laboratory phonology' (a movement focusing on the application of findings in acoustic phonetics to developments in phonological theory and vice versa). With the possible exception of Lieberman and Blumstein 1988, I have generally found the most useful theoretical discussions and overviews in the writings of those who are influenced by the more phonetically based point of view typical of the British schools of phonetics and phonology. [2]

Baart, Joan. 1999. Acoustic phonetics: Chapters 2-5, preliminary edition. In Lingualinks 4.0 LT (CD-ROM). Dallas,TX: SIL.

This work does not exist as a separated printed edition; it is part of the linguistic bookshelf in the Lingualinks CD-ROM library, designed to make basic linguistic references available to field linguists far removed from library resources. Baart has not forgotten what he wrote in his 1996 article; he has made a excellent start on producing 'a systematic overview of the findings of acoustic phoneticians' by writing this 80 page work.

In ch. 2 'Speech waves' he begins with a good basic explanation of sound waves, including a discussion of their correlation to waveform graphs. Next comes a good explanation of how to identify waveform traces of more common phonetic segments, including a discussion of sounds that can blend into each other, common problems with identification, and criteria for determining segment boundaries. With regard to determining segment boundaries (or for that matter, measuring any acoustic parameter), the field linguist should take special note of his concluding remarks:

> Different languages have different inventories of sounds and different possible combinations of sounds. Detailed segmentation criteria will to a certain extent be unique for one language, and will need to be developed and made explicit as a researcher gains experience in segmenting words and sentences uttered in that language.

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[2] After this bibliography was originally submitted for publication I became aware of another book which may be useful to the field linguist. It is Johnson 1997, and it is reviewed in a recent issue of Language (Shahin 1999). I have not been able to examine it personally. It is not too long (169 pp.), yet the list of contents suggest that it covers the basics of acoustic and auditory perception. Shahin 1999 praises the detailed discussion of digital speech processing (this is what the field linguist is doing when they use CECIL or Speech Analyzer), and considers the book as a whole to be 'a current reflection of the field.' Another strength is the use of examples from 'a wide variety of languages' throughout the presentation—most of the other books on acoustic linguistics mentioned here suffer in this regard.

This certainly describes my experience—in fact, it was consoling to see that without any guide except other readings in this bibliography and seat-of-the-pants intuition I had stumbled onto using some of the criteria he suggests. This is in accord with a later observation of the author (and some others in this bibliography): reading CECIL and Speech Analyzer graphs is as much art as science.

Ch. 3 'Sound spectrograms and spectra' explains basic concepts behind the generation of spectrograms (these show variations in frequency across time) and spectra (these show frequencies where the most energy is concentrated), including how programs like CECIL produce these, how to interpret them, and acoustic features of more common sounds as they appear in these classes of diagram. The reader here learns how to measure vowel formant frequencies, how to interpret and display vowel formant data, how to recognize spurious formants and the effects of consonants on vowel formants, and how to describe diphthongs.

Ch. 4 'Voice and aspiration' introduces the effects of breathy, modal, and creaky voice on spectra and waveforms. This is followed by a fairly detailed discussion of how to distinguish between voiced and voiceless segments (mostly using waveform graphs) and recognize aspiration, as well as the significance of voicing onset time. The author notes that acoustic features distinguishing voiced and voiceless segments are language variable. His work is meant to serve as a guide, and the criteria discussed here cannot be set in concrete across languages.

Ch. 5 'Prosody' introduces the field linguist to the interpretation of fundamental frequency and intensity graphs, the effects of segments on intensity and fundamental frequency, and the use of these graphs to analyze prosody. There is a discussion of articulatory, acoustic and perceptual correlates of accent, the use of intensity to determine accent (according to the author it is the 'weakest cue to accent perception...easily overruled by manipulations of syllable duration and F0 [fundamental frequency]' (75)), the use of spectral tilt (very difficult, if not impossible to measure, using CECIL), and other phonetic correlates of 'accent'.

It should be noted that the author defines accent as 'the prominence of a syllable in a spoken utterance, as perceived by a native speaker of the language' (73). He uses Bolinger's definition of stress throughout this discussion: an 'abstract property of a word, specifying a default "landing site" for accent (74).' To practitioners of some of the more recent varieties of American generative phonology it will look like word level and intonational accent are confused in this chapter. The apparent confusion is the result of significant theoretical differences between current approaches to the relationship and analysis of intonational and word-level phonology. The

confused reader should study the first two chapters of Ladd 1996, if the difference in theoretical orientation is preventing them from applying Baart's practical recommendations for the study of prosody using CECIL graphs.

In ch. 5 Baart offers good advice for interpreting segmental duration: it is best to look at overall proportions, instead of absolute differences. There is also a discussion of tone, including criteria to determine if one is dealing with a tone language, and some discussion of the differences in criteria for determining accent between 'pure' tone languages, pitch-accent languages, and languages with neither lexical tone nor pitch-accent.

Baart's brief introduction is the most practically orientated of all the works in this bibliography. The CECIL or Speech Analyzer neophyte should begin here, but the work has much to offer the veteran CECIL user as well.

Catford, J.C. 1977. Fundamental problems in phonetics. Bloomington: Indiana University Press.

Like Laver's volume (see below), this volume provides all kinds of tidbits which turn out to be most useful in interpreting CECIL diagrams. Unlike Laver, Catford devotes a whole chapter to the acoustic phase of speech (Ch. 4); this provides a succinct introduction to acoustic phonetics. The rest of the book makes up for the brevity of this chapter by including abundant data and charts taken from the findings of acoustic phoneticians; there is also a brief chapter at the end of the book on 'instrumental phonetics' which should also be useful to the CECIL neophyte.

This book contains an especially useful discussion of the relationship of articulatory and acoustic descriptions of vowels (Ch. 9); the discussion of the acoustics of sibilants is also extremely useful (Ch. 8). The crowning glory of the latter discussion has to be the diagram 'acoustic spectra of some fricatives pronounced with and without teeth' (155). It is the most effective illustration of extraneous factors which can influence format frequencies that I have seen to date; it serves as a good illustration of Kenstowicz's statement about interpreting spectrograms (see below).

Catford, J.C. 1988. A practical introduction to phonetics. New York: Oxford University Press.

Ostensibly this book has little to do with acoustic phonetics; however, when CECIL found its way to my office table, I was a bit rusty on basic articulatory phonetics. This is the finest, easiest to read, most basic, yet theoretically most up-to-date introduction I found. The main strength of Catford's work lies in the up-to-date phonetic theory, and in the requirement that his readers use their articulatory tract as a phonetic laboratory in order to acquire a feel for all the basic articulatory and phonation mechanisms and

modifications. Once acquired, this basic feel is incredibly useful in the interpretation and transcription of CECIL graphs.

Clark, John, and Colin Yallop. 1995. An introduction to phonetics and phonology. [2nd ed.] Oxford: Blackwell.

The authors include chapters on 'the acoustics of speech production' (206-299), 'anatomy and physiology of speech production' (161-204), and 'speech perception' (301-326). For a field linguist who wants information on other topics discussed in this book, but who doesn't have access to all the works in this bibliography, Clark and Yallop would serve as a worthy substitute for Lieberman and Blumstein 1988 (see below). The discussions in Clark and Yallop tend to be less technical and more detailed than in Lieberman and Blumstein; they are basically an extension of the basic introduction to be found in Fry 1979.

Except for Baart 1999, the discussions of 'acoustic features of prosody' (under 'the acoustics of speech production'), the chapter on speech perception, and pp.328-348 of the chapter 'Prosody' provide the most useful information for studying prosody with CECIL graphs. Used in conjunction with Ladd 1996, Baart's introduction and Clark and Yallop provide the clearest guidelines I have been able to find for coping with prosodic analysis in Chimila, where underlying tonal and surface intonational systems interact extensively.

For those who have interests other than acoustic phonetics, Clark and Yallop provide a nice bonus: a final chapter ('The progress of phonology', 385-421) which briefly traces the development of current phonological theory and succinctly describes various phonological theories currently in practice. The discussion provides a unique overview because of the authors' tendency to put more value on the phonetic input into theory than on the theories themselves.

Fry, D.B. 1979. The physics of speech. (Cambridge Textbooks in Linguistics.) Cambridge: Cambridge University Press.

In spite of the title this little volume is actually an introduction to acoustic phonetics for linguists without any background in physics and precious little background in math; it is exceeded in clarity and simplicity of presentation only by Ladefoged 1996 (see below). The book contains outstanding sections on theory and interpretation of sound spectrographs (Ch. 9) and 'acoustic features of English sounds' (Ch. 10); these chapters provide more useful detail on their respective subjects than other references here, except perhaps for Joos 1948 and Lieberman and Blumstein 1988.

Huffmann, Marie, and Rena Krakow (eds.) 1993. Phonetics and phonology, vol. 5: Nasals, nasalization, and the velum. New York: Academic Press.

Several articles in this anthology are useful to the field linguist engaging in CECIL research. The most useful to me was Shinji Maeda's 'Acoustics of vowel nasalization and articulatory shifts in French nasal vowels' (147-167). It is the most (and indeed only) comprehensive introduction to the acoustics of vowel nasalization which I have been able to find; none of the other more general references in this annotated bibliography provide enough detail which would allow a field linguist to successfully use CECIL in a study of vowel nasalization. The discussion of nasal formant theory and especially the formant diagrams on p.154 will be useful to the field linguist struggling with vowel nasalization.

Another paper in the same volume offers a briefer discussion of 'spectral measures of vowel nasalization' (Rena A. Krakow and Marie K. Huffman, 'Instruments and techniques for investigating nasalization and velopharyngeal function in the laboratory: An introduction': see especially pp.155-165). Abigail C. Cohn's paper 'The status of nasalized continuants' in the same volume (329-365) offers useful information for interpreting the effect of vowel nasalization on consonant spectrograms, and vice versa. Ian Maddieson and Peter Ladefoged's paper 'Phonetics of partially nasal consonants' (251-301) is loaded with wonderful tidbits (including CECIL-like data graphs and spectrograms) which are most helpful in the interpretation of CECIL graphs of this class of phonetic segments.

Joos, Martin. 1948. Acoustic phonetics. Language [Supplement] 24:1-136.

In spite of a fifty year time lag, this is still a classic introduction to acoustic phonetics in the English speaking world. The field linguist without much scientific or mathematical background will find it to be highly technical; furthermore, some of the theoretical discussions will need to be supplemented by more recent works such as Lieberman and Blumstein 1988 (see below).

Two chapters offer crucial information for the field linguist who is trying to sort trash from treasure in CECIL graphs and spectrograms: Ch. 3 'Indeterminacy and perception', which discusses the various sources of 'fuzziness' in spectrograms (chiefly related to speech perception); and Ch. 4 'Consonants and minor vowel features' which is a detailed 'commentary on the spectrogram reproductions'.

Ch. 2 'Further acoustic theory and basic vowel theory' provides useful information on constructing formant charts (see especially pp. 50-54, 59-65;

the latter pages are helpful in understanding idiolectical variations in spectrogram patterns). Bark diagrams are a later form of these diagrams discussed in Geoffrey Hunt's 1995 edition of Interpreting CECIL.

Kenstowicz, Michael. 1994. Phonology in Generative Grammar. Oxford: Blackwell.

Section 4.4 of Ch. 4 in this useful volume offers a succinct introduction to acoustic phonetics; section 4.5 presents an introduction to speech perception (see Lieberman and Blumstein 1988 below). On p.182 the field linguist will find a most useful chart which presents 'acoustic correlates of consonantal features'; on p.183 one finds a list of 'formant frequencies for eight American English vowels' which will also be useful as a comparison of standard when the field linguist begins to study vowel spectrographs. The section on speech perception has a useful discussion of the effect of voiced stops on formant frequencies.

On the interpretation of spectrograms Kenstowicz observes (182):

> It is important to realize that the interpretation of spectrograms is an art that requires experience and guesswork. In spite of considerable research on the problem, speech scientists have not yet succeeded in devising a mechanical method to decode the speech signal.

Ladefoged, Peter. 1996. Elements of acoustic phonetics. [2nd ed.] Chicago: University of Chicago Press.

The intent of this author is to provide a comprehensive introduction to acoustic phonetics for linguistics who have little or no background in the physical sciences, and no mathematical background other than algebra. He succeeds admirably; in fact, his work clarifies much even for those who have an extensive scientific and mathematical background.

Because this work aims to provide a basic, general theoretical discussion, it does not contain as much useful data and charts as some of the other entries in this bibliography; the diagram of English vowel formants in relation to the position of the vocal organs (100-101) is a happy exception. Instead the author adds in his second edition three chapters devoted to the principles and theory of digital processing. No other book listed here offers this kind of detailed, up-to-date theoretical overview, and those linguists who feel a need for a theoretical discussion should go to this author first.

The author considers that an understanding of the principles and theory of digital processing enhances the typical linguist's ability to use programs like CECIL. This is no doubt true, but (as the author also implies) the field linguist who does not want to know will probably get along adequately

without this specialized knowledge so capably presented here, and still glean much from this incomparable book.

Ladefoged, Peter, and Ian Maddieson. 1996. The sounds of the world's languages. Oxford: Blackwell.

The value of this volume for field linguists lies in its comprehensive discussions of possible sounds in the world's languages, with detailed articulatory and acoustic descriptions for most members of each sound category and abundant examples from individual languages. Some discussion of possible phonological roles of the various sounds, and the relationship of their phonetics to the phonology in individual languages is also included. This book is basically a distillation of that series of journals on acoustic linguistics in that good technical library of which the ordinary field linguist can only dream.

The authors assume that readers already have a comprehensive knowledge of acoustic phonetics. The text abounds in spectrograms, and has some waveform diagrams. Most individual field linguists probably will not want to own this volume, unless they specialize in articulatory or acoustic phonetics, or use CECIL heavily. Nevertheless, field libraries and linguistic consultants specializing in phonology or phonetics should definitely have this one on their shelves (and in use).

Laver, John. 1994. Principles of phonetics. Cambridge: Cambridge University Press.

This author does not deal with acoustic phonetics as such in this hefty tome. He does, however, provide a considerable amount of detail concerning articulatory phonetics that I have found useful as I interpret CECIL graphs, and try to relate what appears on the computer screen to what comes of out of the speaker when I punch the replay buttons. The articulatory descriptions often contain useful information on the expected length of segments; the author brings examples from a wide variety of languages, and bases much of his descriptions on work by acoustic phoneticians. I have found the data in the chapter on segmental duration, the discussion of diphthongs, the presentation on timing differences in stop voicing onset, and the chapter on pitch and loudness to be especially useful as I interpret CECIL diagrams for Chimila, and work on consultant jobs which have also involved the use of CECIL data.

The last chapter (Ch. 19) 'Evaluating general phonetic theory' is a revealing discussion of current issues which many field linguists likely do not even realize exist, if they do not specialize in phonetics. These issues are directly pertinent to the interpretation of CECIL graphs, although one can be totally unaware and still interpret such graphs quite successfully.

Lieberman, Philip, and Sheila E. Blumstein. 1988. Speech physiology, speech perception, and acoustic phonetics. (Cambridge Studies in Speech Science and Communication.) Cambridge: Cambridge University Press.

The authors intend this work to be a textbook for linguistics students without background knowledge in physics, anatomy, or psychology. The result is an exposition which lies somewhere in between Fry 1969 and Joos 1948 with respect to technical difficulty. It is a perfect volume for field linguists who want more detail than Fry 1979 or Ladefoged 1996 have to offer, but do not want to take the risk of drowning in the technical discussions of Joos 1948.

Ch. 5 is a detailed discussion on instrumental speech analysis which goes beyond the discussion in Ladefoged 1996, because it is more practically oriented than the latter. In Lieberman and Blumstein's work the discussion covers 'the capabilities and the limitations of the sound spectrograph', techniques for tape recording that minimize distortion', and 'current computer-analysis techniques that can be applied in the acoustic analysis of speech' (51). It contains useful advice that can help a CECIL user determine when it would be wise to use spectrographs in analysis. It goes into even more detail on eliminating distortion in instrumental speech analysis than does the CECIL documentation. A major strength of this discussion is the consistent detailed referral to the physiological, physical, and acoustical factors which can contribute to distorted graphs.

Another strength of the work is a chapter on phonetic theories (Ch. 8); the comments on a chapter on the same topic in the entry for Laver 1994 apply here.

Stevens, Kenneth N. 1998. Acoustic phonetics. Cambridge, MA: MIT Press.

The author is an acoustic phonetician known for his 'quantal theory' of speech production;[3] moreover, because of his qualifications, his book is

[3] According to Crystal 1997 the 'quantal theory' is based on the 'quantum', 'a zone of articulatory performance within which the results of minor articulatory variation are not auditorily perceptible' (318); the theory proposes that these zones exist, and that 'a small shift outside of this zone will produce a large acoustic change.... It is argued that articulation is evolutionarily organized to make maximum use of the vocal tract's ability to produce such changes', and it is these changes which lead to 'the development of phonological distinctiveness' (318). (A brief definition can also be found on p.295 in Clark and Yallop 1995; Lieberman and Blumstein 1988 offer more extended discussions on pp 171-6 and 184-188.) The more common acoustic theory of speech production is Gunnar Fant's 'source filter theory', discussed in varying degrees of detail in most of the books in this bibliography.

likely to become a standard in the field. Therefore his work should not be ignored by anyone who is seriously pursuing expert competence in acoustic phonetics. According to the blurb in MIT Press' Linguistics 2000 catalog he 'begins with a review of the anatomy and physiology of speech production, then covers source mechanisms, the vocal tract as an acoustic filter, relevant aspects of auditory psychophysics and physiology, and phonological representations.' In addition he examines in detail 'vowels, consonants, and the influence of context on speech sound production.'

This all looks good, but I was disappointed to read the next line: 'Although he focuses mainly on the sounds of English, he touches briefly on sounds in other languages.' The cost of the hardback volume, its length (672 pp.), and the statement about being devoted mostly to English acoustic phonetics has deterred me from examining a copy to find out just how brief 'briefly' is. Perhaps if MIT Press decides to bring this one out in paperback it will become accessible to a wider audience that includes field linguists.

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REVIEWS

Chomsky: Ideas and ideals. By NEIL SMITH. Cambridge: Cambridge University Press, 1999. Pp. 268. Cloth $54.95, paper $19.95.

Reviewed by CHERYL A. BLACK
SIL—Mexico

Personally, I have always found history to be simply a list of facts and dates as presented in textbooks, but a biography of one of the principle figures makes history come alive as I understand how the events affected people's lives. Similarly, my approach to linguistic theory has been to study the problematic data in order to understand the reasons behind the theory. But although I studied Chomsky's linguistic theories throughout graduate school and have applied the concepts to various languages and also taught them to others, I never understood the bigger picture of how Chomsky's ideas fit into science and society as a whole until reading this book.

This is not a biography of Chomsky; details about his life are presented in a single paragraph in the introduction. Instead, it is about his ideas on language and the study of the mind. His work in linguistics, psychology, and philosophy has been systematically innovative and also controversial and misunderstood. Neil Smith attempts to clarify the misunderstandings in a simplified, comprehensible presentation.

The book is divided into five chapters. Ch. 1 'The mirror of the mind', begins by showing that language is what distinguishes humans from animals. Therefore, our linguistic ability provides a view into our minds. Chomsky brought linguistics into the mainstream of true science. While linguistics had long been recognized as the scientific study of language, this study was limited to the realm of classificatory science. Chomsky claims that linguistics is fully scientific by providing a general theory which explains why languages are the way they are (i.e. that each language is a particular example of a universal faculty of the mind) and by showing that the theory has testable hypotheses.

Ch. 2 'The linguistic foundation' provides a very interesting, non-technical but data-driven overview of the main ideas in Chomsky's theories. It takes a historical perspective, developing each idea, and moving up to the current Minimalist framework. The current theory is understood best by seeing the argumentation involved and the steps that led to its proposal.

Ch. 3 'Psychological reality' is defined by Chomsky as the truth of a certain theory. Chomsky argues that we have grammars in our heads, and that

language processing, a child's acquisition of their first language, and language breakdown in pathology situations give evidence for this. A major achievement of the Chomskyan paradigm is that no one believes any longer that languages can differ from each other in unlimited and unpredictable ways. His theories of universal grammar and parameter setting give insights into 'Plato's problem' of how a child can learn a language.

Chs. 4 and 5 deal with Chomsky's more controversial views of philosophy and politics. Though the author explains Chomsky's views well, there are many points which will be controversial, especially his radical rejection of authority, his agnosticism, and his unbelief in moral absolutes. Of course, these views do not make Chomsky's scientific contributions any less valid.

This book presents Chomsky's ideas and ideals as a coherent whole. Though direct applications to field work are not obvious, besides a better understanding of the development of his linguistic theories, some indirect applications come to mind. One interesting use for field linguists would be as an exercise in cultural anthropology: understanding the world view of someone you do not agree with. A second area of application would be for those studying and teaching second language acquisition: if indeed there is psychological reality to a universal grammar with parameter settings which helps a child acquire a language, then it is plausible that a method of teaching following this model would help an adult learn a second language, even though the critical period of being able to innately determine the parameters has passed.

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Intransitive predication. By LEON STASSEN. (Oxford studies in typology and linguistic theory.) Oxford: Clarendon Press, 1997. Pp. xvi, 771. Cloth $145.00.

Reviewed by MICHAEL BOUTIN
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There is no way that a short review can do justice to this very long account of intransitive predicates. To assist his readers in dissecting and absorbing the contents of this book, Stassen (S) provides some very useful advice at the end of Ch. 1. He invites readers to first consult his Conclusion in Ch. 15 in order to determine whether or not they want to set out on the long road through this book and what route they should take. While the road is long, it is also very interesting and a major contribution to our understanding of

intransitive predicates. Not only is S is to be commended for the amount of data on which his conclusions are based, but also for the degree of detail that makes this book an exemplar for linguistic typology.

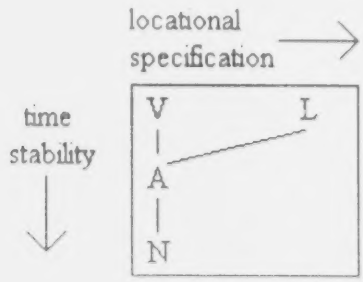
S's data base sample consists of 410 languages which have a fairly even genetic and areal distribution (8).[1] The domain of his inquiry is non-embedded propositions that consist of a one-place predicate and its argument. Such propositions are typically encoded as intransitive main clauses. S restricts his inquiry to intransitive declarative main clause predicates with a definite subject (11). He assumes that intransitive predicates can universally be divided into four semantic categories: event predicates, e.g. 1a; property-concept predicates, e.g. 1b; class-membership predicates, e.g. 1c; and locational predicates, e.g. 1d. The data base consists of the equivalents of the English sentences in 1 for each language in the sample (611).

(1) a. John walks.
b. John is tall.
c. John is a carpenter.
d. John is in the kitchen.

S's aim is to document in the form of a typology the ways in which this four-way universal subcategorization of intransitive predicates is formally encoded. He then provides explanations for the cross-linguistic generalizations that can be formed on the basis of his typology (611).

S admits that the boundaries between the four semantic predicate categories are not sharply delineated; instead, all predicates have a focal point around which prototypical members tend to cluster (14, 578). S maps the semantic space for intransitive predicates as in 2.

(2) Universal semantic map for intransitive predicates



The semantic space in 2 is determined by universal principles. Time stability forms one of the coordinates on the semantic map for intransitive predicates. Class-membership predicates (N) are most time stable, event predicates (V) are least time stable, and property-concept predicates (A) occupy a central position between the two. Although locational predicates (L) are more difficult to integrate into the time stability hierarchy, they are separated from both event predicates and class-membership predicates by property-concept predicates. Because locational predicates are less time stable than either property-concept predicates or class-membership predicates, they occupy the same position as event predicates on the vertical axis.

Natural languages employ different formal, morphosyntactic encoding strategies for different semantic classes of intransitive predicates. Part One (Chs. 1-4) shows that languages employ a maximum of three different strategies for the formal encoding of the domain of intransitive predicates (121). Property-concept predicates never have an encoding strategy of their own; instead, their encoding is always taken over by one of the other three predicate types. S compares the domain of intransitive predicates to a battlefield in which three kingdoms (V, N, and L) have a strategy whereby they try to conquer as much of the domain as possible with property-concept predicates being sort of a 'no man's land' between the three competing strategies. In some cases, the battle leads to the defeat of one (or even two) of the other kingdoms which results in the loss of the defeated strategy. In other cases, a truce is reached which results in conflicting strategies.

A strategy is a specific, distinctive, morphosyntactic procedure for encoding one or more categories of intransitive predicates. English uses two different encoding strategies. Event predicates like *walk* in 1a allow marking for third-person subject agreement by the suffix *-s*, whereas the other three predicate categories lack this encoding. Furthermore, property-concept predicates (e.g. 1b), class-membership predicates (e.g. 1c), and locational predicates (e.g. 1d) require a copular verb.

Languages may vary in the number of the strategies they employ and in the distribution of the strategies they employ (26, 151); however, S predicts that no language can employ more than three different strategies (30). S presupposes a set of criteria which guide the decision as to whether to group or separate semantic predicate categories in terms of strategy selection (27). By comparing the structural features of each language in the data base, S was able to formulate prototypical encoding strategies for each of the three

[1] The appendices list the sample languages according to alphabetical order, areal and genetic affiliation, and the intransitive predicate typology that S establishes.

warring strategies. (Since property-concept predicates never have an encoding strategy of their own, they do not have a prototypical encoding strategy.) For example, the prototypical features of the event predicate strategy are shown in 3.

(3) Prototypical Features of the Event Predicate Strategy
   a. the presence of person agreement if the language allows person marking at all;
   b. the absence of supportive items; and
   c. a specific negation strategy (50).

According to S, deviation from the prototype strategy is evidence that the prototype has been or is in the process of being taken over by another strategy. On the basis of prototypical encoding features for each of the three warring strategies, S establishes a number of criteria for distinguishing the different strategies. For instance, since the prototypical encoding strategy for event predicates includes person agreement between the verb and the subject (i.e., 3a), S formulated the Agreement Criterion in 4.

(4) The Agreement Criterion
   If a language has person agreement, any predicational strategy in that language which does not employ the same system of person marking as verbs is nonverbal (38).

Tiwi (Northern Australian) is an example of a language in which the Agreement Criterion identifies a nonverbal strategy. Event predicates in Tiwi are obligatorily marked for person agreement as in 5a; however, person agreement is not allowed if the predicate is a property-concept as in 5b or a class-membership predicate as in 5c. Application of the Agreement Criterion leads us to conclude that in Tiwi property-concept predicates and class-membership predicates have a coding strategy which is distinct from event predicates.

(5) Tiwi (Australian)
   a. Ji-pauliɣi.
      3SG.MASCULINE.PAST-fall
      'He fell.' (Osborne 1974:70; Stassen 1997:38)
   b. Tuŋkwaltiriŋa pumpuka.
      stringy.bark good
      'The stringy bark is good.' (Osborne 1974:60; Stassen 1997:39)
   c. Purukupaɹli maɹntina.
      Purukuparli boss
      'Purukuparli is boss.' (Osborne 1974:60; Stassen 1997:39)

Because 4 is only applicable in languages which have person agreement, two other criteria are needed to distinguish the encoding strategy for event predicates as opposed to other types of predicates. These two other criteria are shown in 6 and 7.

(6) The Auxiliary Criterion
If, in a language with nonsupported verbs, a predicate category needs a support:.e item, that category will be rated as a case of nonverbal encoding (42).

(7) The Negation Criterion
If the encoding of a category of intransitive main predicates differs in its negation strategy from the negation strategy of predicative verbs, then that category must be rated as being encoded nonverbally (45).

Because Mandarin Chinese lacks person agreement, the Agreement Criterion is irrelevant. While event predicates and property-concept predicates in Mandarin have nonsupported forms, class-membership predicates require the copular *shì*. Thus, application of the Auxiliary Criterion leads us to conclude that in Mandarin class-membership predicates have an encoding strategy which is distinct from event predicates and property-concept predicates.

In English, as noted above, the contrast between event predicates and other predicates follows from application of both the Agreement Criterion and the Auxiliary Criterion.

The Negation Criterion is supplemental in that it is only used when neither the Agreement Criterion nor the Auxiliary Criterion can distinguish event predicates from other predicates. For example, both Tagalog and Indonesian are Western Austronesian languages that have nonsupported event predicates and lack any form of agreement. In both languages, property-concept predicates (e.g. 8b and 9b) and class-membership predicates (e.g. 8c and 9c) exhibit the same encoding features as event predicates with respect to the Agreement Criterion and the Auxiliary Criterion.

(8) Tagalog (Austronesian, Central Philippines)
a. Naligo si Juan.
bathe NOM Juan
'Juan took a bath.' (Schachter & Otanes 1972:541; Stassen 1997:47)
b. Bago ang bahay.
new NOM house
'The house is new.' (Schachter & Otanes 1972:64; Stassen 1997:47)

c. Arista ang babae.
actress NOM woman
'The woman is an actress.' (Schachter & Otanes 1972:61; Stassen 1997:47)

(9) Indonesian (Austronesian, West Indonesian)
a. Saya duduk.
1SG sit.down
'I sit/sat down.' (Kähler 1965:168; Stassen 1997:47)
b. Muka-nya manis.
face-her sweet
'Her face is/was sweet/pretty.' (Kähler 1965:56; Stassen 1997:47)
c. Si Aman pelayan.
ARTICLE Aman servant
'Aman is/was a servant.' (Kähler 1965:45; Stassen 1997:47)

Tagalog, however, has a uniform strategy for negation, whereas Indonesian has a differentiated encoding of intransitive predicates under negation (48). In Indonesian, event predicates (e.g. 10a) and property-concept predicates (e.g. 10b) are negated with *tak/tidak*, whereas class-membership predicates (e.g. 10c) cannot be negated with *tak/tidak*, but instead are negated with *bukan*. Thus, according to the Negation Criterion in 7, in Indonesian class-membership predicates and event predicates have a different encoding strategy.

(10) Indonesian (Austronesian, West Indonesian)
a. Ia tidak datang.
3SG NEG come
'He did not come.' (Kwee 1965:10; Stassen 1997:48)
b. Si Ali tak berani.
ARTICLE Ali NEG courageous
'Ali is not courageous.' (Kähler 1965:56; Stassen 1997:48)
c. Minah bukan guru.
Minah NEG teacher
'Minah is not a teacher.' (Kähler 1965:56; Stassen 1997:48)

Verbal takeover occurs in a language when property-concept predicates, class-membership predicates, or locational predicates are encoded in the same manner as event predicates in terms of the Agreement Criterion in 4, the Auxiliary Criterion in 6 and the Negation Criterion in 7. For example, in Indonesian, property-concept predicates (e.g. 9b and 10b) have undergone verbal takeover since they are treated the same as event predicates (e.g. 9a and 10a) in terms of the criteria in 4, 6 and 7. In Tagalog, both property-

concept predicates (e.g. 8b) and class-membership predicates (e.g. 8c) have undergone verbal takeover since they are treated the same as event predicates (e.g. 8a) in terms of the criteria in 4, 6 and 7. In English and Tiwi, verbal takeover has not occurred since neither property-concept predicates (e.g. 1b and 5b respectively) nor class-membership predicates (e.g. 1c and 5c) are treated the same as event predicates (e.g. 1a and 5a) in terms of the criteria in 4, 6 and 7.

After accounting for the features of verbal, locational and nominal strategies in Chs. 2 and 3, S sets up his typology of intransitive predicates in Ch. 4. S's typology consists of eighteen possible patterns (fifteen of which are actually attested) that he divides into three major groups: A-LANGUAGES are languages that show some form of verbal takeover; B-LANGUAGES are languages that do not show any form of verbal takeover; and C-LANGUAGES are languages which lack a verbal strategy (125). The majority of the sample belongs to either the A-LANGUAGES group or the B-LANGUAGES group. For example, Indonesian and Tagalog belong to the A-LANGUAGES group since they involve some form of verbal takeover, whereas English and Tiwi belong to the B-LANGUAGES group since they do not show any form of verbal takeover, yet they do have a verbal strategy.

Within each of the three groups, languages are further subdivided according to their takeover pattern. For example, since Tagalog employs one encoding strategy for all four semantic predicate categories (24), its pattern is: A VVVV. This pattern indicates that Tagalog is an A-LANGUAGE in which the verbal strategy V has taken over property-concept predicates, class-membership predicates, and locational predicates. In contrast, Indonesian's pattern is: A VVNV. This pattern indicates that Indonesian is an A-LANGUAGE in which the verbal strategy V has taken over property-concept predicates and locational predicates.

Part Two (Chs. 5-8) deals with switching or 'partial takeover'. S shows how it is possible for encoding to be split up between two (or even three) different strategies.

Part Three (Chs. 9-13) argues that tense-marking of predicates is a crucial parameter in the explanation of the typological variation in intransitive predicate encoding (54). As stated above, property-concept predicates never have an encoding strategy of their own; instead, their encoding is always taken over by one of the other three predicate strategies. Most commonly, either a verbal takeover or a nominal takeover occurs. In Part Three, S seeks an explanation for why property-concept predicates in some languages undergo verbal takeover, whereas in other languages they undergo nominal takeover. S found a correlation between tense and takeover. On the one hand, if a language is tensed, then property-concept predicates most likely

undergo nominal takeover. On the other hand, if a language is non-tensed, then property-concept predicates most likely undergo verbal takeover.

Stassen has produced a book that contains very interesting theoretical claims that are based on a wealth of data. For a book this size, there are very few typos in the text itself; for example, pages 159-60 refer to Biggs (1989), whereas pages 721 and 735 refer to Biggs (1969). Potential errors in the data (e.g. the gloss for Indonesian *dia* in 22a of p. 249 should be '3SG', not '3PL') are harder to judge since readers will not be familiar with most languages. At any rate, orthographic errors or inconsistencies in glosses do not detract from S's main arguments. Inconsistencies in the labeling of grammatical morphemes are almost unavoidable when you start citing multiple sources for related languages. While other inconsistencies show up if you really dig around (e.g. Tagalog is a 'Philippine' language on p. 24, a 'Northern Philippine' language on p. 47, and a 'Central Philippine' language on p. 669), S's claims are very clear. This book is an important contribution to our understanding of intransitive predicates. I recommend it for every SIL entity library.

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The comparative method reviewed: Regularity and irregularity in language change. Edited by MARK DURIE and MALCOLM ROSS. Oxford: Oxford University Press, 1996. Pp. 321. Cloth $75.00.

Reviewed by KARL FRANKLIN
SIL International—VP Academic Affairs

In this comprehensive study the editors outline (Ch. 1) the basic assumptions of the comparative method, which are then amplified in the nine chapters which follow. Durie and Ross summarize the comparative method as a set of instructions (6-7, but here somewhat abbreviated):

1. Determine a set of genetically related languages, i.e., a family;
2. Collect putative cognate sets for the family;
3. Work out the sound correspondences and note the irregularities;
4. Reconstruct the proto-language, both sound correspondences and morphological paradigms;
5. Establish innovations shared by groups of languages within the family;
6. Tabulate the innovations to arrive at an internal classification of the family;
7. Construct an etymological dictionary, tracing borrowings, changes, etc. for the family.

This sounds straightforward enough, but the rest of the book tells why it is not. In point 3, the authors remind the reader that to 'note the irregularities' is a difficult task. This is because the causes of irregularity are 'many and complementary' (13), including such things as innovation, semantic change, communicative exigency (the avoidance of homophony), taboo and bilingualism–for a start. Such irregularities can be more easily understood from a speaker-oriented framework (i.e. sociolinguistics), than from a language-oriented framework (31). The authors therefore adopt the prevalent sociolinguistic position that 'the language system of a community entails orderly variation,' for example, certain sounds are less prone to change than other sounds and speakers have various motivations for language change, not simply prestige.

Ch. 2, 'The comparative method as heuristic' by Johanna Nichols illustrates that 'even the extensive Proto-Indo-European or Proto-Slavic vocabulary now reconstructed, complete with multiple regular sound correspondences, serves only as a secondary confirmation of the genetic relatedness of Indo-European or Slavic' (64). This is because the classical method of comparison and reconstruction is limited to a time frame when the particular language must have remained intact and depends on lexical comparison. This, Nichols claims, is not a heuristic except in a limited sense of establishing additional daughter languages into an existing family. The classical method may also imply more genetic likeness than does in fact exist in the world's languages.

Ch. 3, 'On sound change and challenges to regularity' by Lyle Campbell discusses seven topics that challenge the assumption that sound change is regular: 'sound symbolism, onomatopoeia and affective/expressive symbolism, avoidance of homophony, morphologically conditioned phonological changes, areal linguistic borrowing, language death, and questions concerning the nature of change in so-called exotic speech communities' (72). He argues that these do not represent true exceptions to the regularity of sound change.

Ch. 4, 'Footnotes to a history of Cantonese: Accounting for the phonological irregularities' by John Newman focuses upon the unusual types of irregularity that result from taboo, the influence of the written characters and some rule interactions.

Ch. 5, 'Early Germanic umlaut and variable rules' by Mark Durie applies the statistical technique of variable rule analysis to account for the exceptions that have occurred in the long history of the Germanic umlaut. One of Durie's methodological conclusions is that it is 'not always necessary or even correct to divide cognate sets into "regular cases" and "exceptions"' (130). He claims that a sound change may be subject to systematic phonological conditioning and yet not be categorically regular in the classical sense.

Ch. 6, 'The Neogrammarian Hypothesis and pandemic irregularity' by Robert Blust uses the pandemic disease metaphor to highlight the abnormal phonological development that is characteristic of the Austronesian languages. In particular, he examines, medial prenasalization and voicing crossover in velar stops and his conclusion is that 'irregularity is not mere chaos. Rather, ...[it] appears to be an integral part of the natural process of language change' (153).

Ch. 7, 'Regularity of change in what?' by George W. Grace provocatively states that 'we cannot be sure why the comparative method works when it does work until we also know why it does not work when it does not' (157). Grace examines a case in which Dempwolff used three particular languages to provide all of the phonemic distinctions required for the protolanguage. But, according to Grace, if these three were the 'right' ones there are others that are equally 'wrong', i.e., their sound correspondences are difficult to establish due to few cognates. This leads to his conclusion that evidence from such languages makes it 'virtually useless' to reconstruct Proto-Austronesian or protolanguages of any of its subgroups. In Grace's terms, the process of the comparative method is 'a going-back-and-forth between the task of identifying cognates and that of identifying sound correspondences,' a task that is not easily separated (165). He then outlines a straightforward set of procedural steps, but then demonstrates why he does not succeed in getting the kinds of reconstructions that would have allowed him to do further stages of reconstruction—leading back to his task of establishing Proto-Oceanic.

Ch. 8, 'Contact-induced change and the comparative method: Cases from Papua New Guinea' by Malcolm D. Ross examines two residual areas: the Takia on Karkar Island near Madang, and the Maisin, on the north coast of the Oro Province. The latter represents the so-called Papuan Tip cluster of languages, and seems to have been subject to a great deal of borrowing.

Takia, on the other hand, is one of two island languages, one Papuan (Waskia), and the other Austronesian, which reflects some Papuan characteristics. Ross coins the term 'metatypy' (182) to deal with such changes in structural typology, a feature that (along with language shift) interferes with the normal Austronesian correspondences he expects. In addition, features of simplification and added complexity, such as processes of elision and assimilation, have resulted in phonological compactness, and this too causes irregularities in sound correspondences. By systematically examining the structural resemblances, Ross concludes that regular processes govern the irregularities. Taking these processes into account allows him to attempt a more serious picture of the reconstruction of culture history.

Ch. 9, 'Reconstruction in morphology' by Harold Koch suggests a set of procedures for determining a typology of morphological changes, with comments on the reconstruction procedures that follow from each. Koch uses case studies, primarily from the Arandic subgroup of Central Australia, to illustrate the procedures. He discusses the development of allomorphy, including basic and derived allomorphs, as well as changes in conditioning, loss and redistribution of allomorphs. Morphemes may change their semantic function such that several subtypes arise according to the shift or loss of grammatical meaning. In addition the morphosyntactic status of a morpheme may change, such as a free word becoming bound, a bound morpheme becoming an affix, a phrase becoming an affix, or an affix becoming a separate morph which can be reanalyzed as part of another morph. According then to the typology Koch outlines, several kinds of change can occur involving loss of lexical meaning, affixation, certain morphophonemic changes, erasures of boundaries, and so on. 'Successful reconstruction depends not only on the existence of "cognate" morphs...but also on the linguist's understanding of what kinds of plausible changes can be expected in the particular domain of morphology at issue' (248).

Ch. 10, 'Natural tendencies of semantic change and the search for cognates' by David P. Wilkins focuses on the problem of demonstrating semantic relatedness between forms that are presumed cognates. Wilkins presents a semantically organized word list that can be used for comparative research (284), a list that is based on the concept of person-parts changes, that is, 'interfield metonymic changes' (to slap > palm of hand), 'interfield metaphoric changes' (boiled rice > brain), 'intrafield metonymic changes' (finger > hand), and 'intrafield metaphoric changes' (cheeks > buttocks). Illustrations are given from a number of Australian languages. Wilkins concludes that natural semantic shifts can be demonstrated crosslinguistically, that the person-part domain explicitly shows the most

commonly attested chains of related changes, and that the natural tendencies of semantic change can be used early in the comparative method.

The Comparative Method Reviewed is a book that contains a detailed and practical outline of comparative-historical linguistics, just as the title promises. Although it is a considerable effort to follow most of the detailed examples of particular languages and the complexities of the so-called irregularities, it is well worth the effort. As the authors tell us at the beginning of the book (31), patterns of irregularity 'can reveal a great deal about linguistic prehistory'. As odd as it may sound, the main contribution of the book is that it insists that there is 'regularity of irregularity' in reconstructing the phonology, morphology and semantics of a group of languages.

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About language: Tasks for teachers of English. By Scott Thornbury. Cambridge: Cambridge University Press, 1997. Pp. 269. Cloth $59.95, Paper $22.95

Reviewed by CHERYL JENSEN
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It is not enough for a teachers of English to be able to speak and understand English; they also need to know a great deal about the way the language functions. This does not mean that they must teach their students every single thing that they themselves know about the language, but without this knowledge they do not have the basis for teaching effectively. It is the purpose of this book to increase the language awareness of teachers (and teachers-to-be) through the investigation of samples of language.

Believing that people learn most effectively through discovery, the author has organized this book to be used inductively. The first section consists of 28 units of Tasks, followed by a second section of Keys and commentaries to each unit of Tasks. Except for the first three units, which are in a sense introductory, the units deal with increasingly larger units of language: phonology (units 4-8), word formation (unit 9), lexical meaning (unit 10), word classes and phrases (unit 11), sentence structure (units 12-13), negatives and questions (unit 14) the verb phrase (units 15-21), the noun phrase (units 22-23), adjectives and adverbs (unit 24), prepositions and

phrasal verbs (unit 25), cohesion (unit 26), texts (unit 27) and conversations (unit 28). These units can also be studied in the reverse order.

The types of tasks, or activities, included in each unit are:

- Identification/recognition: 'Find all examples of X.'
- Categorization: 'Classify all the examples of X.'
- Matching tasks: 'Match examples X with definitions Y.'
- Explanation/interpretation: 'Explain all examples of X.'
- Evaluation: 'Assess the usefulness of this exercise to practise item X.'
- Application: 'Design an exercise to practise item X.'

For example, in Unit 11 (Word classes and phrases), the following tasks are given:

1. Word classes: Match the term (noun, pronoun, verb, etc.) with its definition.
2. Word classes: Find an example of each word class in the following text.
3. Groups: One sentence from the text in task 2 is divided into word groupings (phrases). Choose which of the four possible analyses of word groupings is the best.
4. Phrases. Match the phrase type (NP, VP, AdjP, AdvP, PP) with the appropriate example.
5. Phrase heads. Identify the phrase type in examples from text, and identify the head of each.
6. Modification. Identify examples of premodification (preceding the head) and postmodification.
7. Phrases. Identify prepositional phrases.
8. Phrases. Identify phrase types in authentic text.

Upon completion of the tasks in this unit, the student refers to the comparable unit in the Key and commentaries section for clarification.

I like the methodology used by the author and think the approach would be useful in a course of introductory linguistics as part of a review of basic language concepts (especially grammatical). As a field linguist, I am interested in trying the methodology used in this book to teach language awareness to native speakers of the indigenous language which I am studying.

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The structure of tone. By ZHIMING BAO. Oxford: Oxford University Press, 1999. Pp. 252. Cloth $55.00.

Reviewed by ANNE GARBER KOMPAORÉ
SIL—Burkina Faso and Africa Inter-Mennonite Mission

When I picked up this book I got excited. Finally a book all about tone! As I thought about it further, it occurred to me that since Pike's (1948) production, comprehensive published works on tone theory and tone phenomena by a single author have been rare[1]. There are several dissertations and books that describe tone in specific languages, a number written on Chinese languages, and a plethora of articles in journals, as well as some very informative books with a collection of articles[2]. So the mere sight of the title filled me with great expectations.

But I was somewhat disappointed—not by the quality of Bao's presentation of the subject matter—but by the somewhat more narrow scope than the book's title leads us to believe. For while Bao's survey of the development of tone theory is excellent, and is an excellent educational resource on the topic, he applies his proposed theory solely to Asiatic languages. Most of his data comes out of Chinese languages. He makes only passing reference to phenomena found African languages, with no real attempt to grapple with the theoretical implications for downstep, for example, and not even a mention of American Indian languages. Therefore, in my opinion, the title should have had an accompanying subtitle which more realistically defined the scope of the study.

B spends considerable space arguing how his theory explains phenomena in numerous Chinese languages. (He uses data from 11 Asiatic languages.) He spends less space exposing weaknesses of other theoretical approaches and demonstrating how his approach is better than previous theoretical approaches.

B's presentation was very clear. He was careful to present background information. Having more familiarity with African languages and very little knowledge of Chinese languages, I found the book very helpful for better understanding tonal phenomena of Chinese dialects. B provides background information on Chinese dialects (in the introduction), explains traditional Chineses linguistics approaches to tone, and gives pertinent diachronic information, all of which serves as a launching pad for his proposed theory.

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[1] An exception is Pulleyblank's *Tone in Lexical Phonology* (1986)

[2] For example, Fromkin (1978) *Tone, A Linguistic Survey* and van der Hulst and Snider (1993) *The Phonology of Tone: The Representation of Tonal Register.*

Ch. 2, a survey of tone theories, while not exhaustive, is informative. Seventeen subsections of the ch. are devoted to explaining the development of tone theory to the present day. Throughout the book B references the wide range of literature available on tone, with the one glaring exception of van der Hulst's and Snider's (1993) *The Phonology of Tone*.

Ch. 3, 'The Representation of tone' demonstrate B's autosegmental framework, dealing first with the nature of tone features. The literature on tone has long maintained the notion that there are two features, High and Low. (Yip 1980 speaks of Upper and Raised). Certain authors have given them plus and minus values producing a potential output of four levels of tone. Others have rather proposed two underlying tiers in the tonal geometry producing again four underlying levels. B presents another variation of the theme, having the goal to represent the contour tones in Chinese with underlying features rather than simply being a combination of level tones (as has been demonstrated for the large majority of African languages).

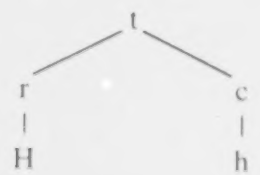
With this goal in mind, B proposed the binary features 'stiff' and 'slack' in an attempt to link phonetic and physiological observations of the vocal cords to a potential theoretical model, taking into account the fact that in a number of languages, tone is often lowered after voiced obstruents. Unfortunately, his hypothesis is not any better substantiated by physiological facts as we know them than earlier hypotheses by Halle and Stevens (1971). In other words, by his own admission, Bao recognizes that the details of the articulatory processes involved in pitch production are not well known, whether one or more muscles are involved. Therefore pitch features even in Bao's theory do not have a solid physiological base.

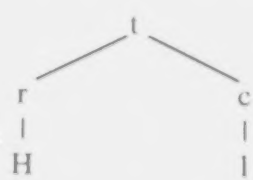
B assigns the features stiff and slack to separate nodes. The node which supports the stiff feature is called the register node, while the slack feature is represented by the contour node. The register node assigns the tone into a high ('H') or low ('L') register ([+stiff] or [-stiff]), while the contour node provides further details within each register. [-slack] (otherwise represented as 'h') within the high register means that the pitch level is at the upper end of the high register, while [+slack] ('l') means that it is at the lower end. In B's theory, contour node features can branch, meaning that 'h' followed by 'l' and linked to the same contour node represents a falling contour within the specified register. The outcome of the theory is that one can have for example, an underlying falling contour within the high register or a falling contour within the low register, but that there is no underlying falling contour that starts at the highest pitch of the 'H' register and falls to the lowest pitch of the 'L' register. If there is such a contour phonetically the theory demands that it be a combination of two underlying registers, i.e. two level tones. While Chinese languages which possess these purported

underlying contours, do rarely allow for rising or falling contour tones that go from one end to the other of the pitch range of the language (51 or 15) . African languages, in which contour tones have been analysed as being a combination of two level tones, do very frequently have falling or rising tones which cover the entire pitch range of the language.

B quickly abandoned the use of his terms 'stiff' and 'slack' and throughout the remainder of the book, he uses the following terms: H register [+stiff], L register [-stiff], h contour [-slack], l contour [+slack]. His proposal then allows for four even tones (two within each register), two rising tones and two falling tones (one within each register), as illustrated below (cited from B's illustration in example 9, p. 48):

(1) Even tones (t=tone, r=register, c=contour, H=register [+stiff], L=register [-stiff], h=contour [-slack], l=contour [+slack])

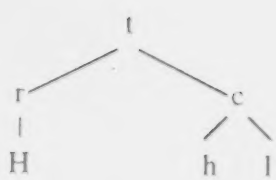
i. 

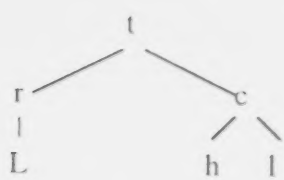
ii. 

iii.

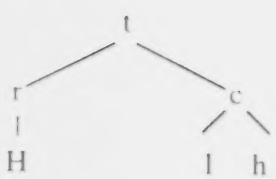
iv.

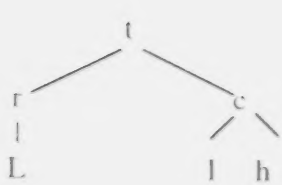
(2) Falling tones

i. 

ii. 

(3) Rising tones

i. 

ii. 

B then demonstrates that to empower the spreading (or assimilation) of the entire tone (register AND contour nodes together), or the register node separately, or the contour node, or of the features themselves, explains certain facts in Chinese languages. He devotes considerable space to data from numerous languages to prove assimilation.

Ch. 4 'The autosegmental nature of tone' further argues that tone is indeed autosegmental by again providing ample data from Chinese languages. B provides examples of tonal morphemes, i.e. morphemes without a segmental base, as well tones which remain after segmental deletion. He also argues that an autosegmental approach to tone also enables a language such as Wuyi where high and low tone registers are in direct correlation to voiceless and voiced obstruents, to generate the rule of 'high register spread' and to follow it by consonant devoicing, i.e. the raised register triggers devoicing of the preceding consonant. He calls this the 'bridge effect' because one element assimilates 'into a nonadjacent element in one plane via some operation on a separate plane' (138).

In Ch. 5, 'Tone in phonological representations' B argues that tone has a dual nature—it is both suprasegmental and segmental. This dual nature must be reflected in the formal structure. He then discusses how tone is to be represented within autosegmental geometry and concludes with evidence from *fanquie* languages[3], that the best autosegmental structure is where 'tones form an autosegmental tier on the syllabic plane' (159).

B then deals with the issue of the segmentalization of tone, that is, the final linking of tone to segmental elements to produce a phonetic output. To illustrate this, he discusses data from Songjiang where voiced/voiceless obstruents correlate with high and low tone registers respectively. The order of tonal processes are presented: tone adjustment, tone mapping, tone sandhi rules and segmentalization. Both tone adjustment and segmentalization deal with matching the consonant type to tone register. For tone adjustment, it is assumed (but not argued) that certain tones are underlying in the lexicon, and that they are lowered or raised depending on the type of consonant (voiced or voiceless) present. At the other end of the derivation when tone is segmentalized after tone sandhi and certain tones have been raised or lowered, another rule is invoked which does just the opposite: Tone devoices or voices preceding obstruents. The attentive reader immediately asks: 'What is the justification for the posited underlying tone?' And what about the possibility that the tones that are presented at the tonal mapping

[3] Fanquie languages are coded adaptations of Chinese languages, where the consonant and vowel of a monosyllabic word are separated; the consonant receives a new vowel, and vowel receives a new consonant, creating a disyllabic word. e.g. *ma* > *mo pa*

stage (after the supposed tone adjustment stage) be posited as underlying and obstruents be underspecified for voicedness? It would seem more economical to me to posit an underlying tone which would undergo any tonal sandhi before specifying the voicedness of the obstruent at the end of the derivation, rather than have two rules, one at the beginning where a consonant influences a tone, at the other at the end where the tone influences the consonant.

The last section of Ch. 5 briefly formalizes the phonological processes of tone sandhi: assimilation (spreading and delinking), dissimilation, contour formation and feature filling (insertion), and contour simplification (deletion).

Ch. 6 discusses the formal status of the mid tone. First B deals with the proposed universal that mid tone is a default tone. He explains that data in Chinese languages reveal no particular tone as default tone. Rather default tones are language specific.

B also deals with postulated dual nature of mid tone. The phonetic output 33, can be analysed as 'H,l', that is 'High register and low even contour', or it can be analysed as 'L,h', that is 'low register and high even contour'. B illustrates how this dual representation solves analytical problems in several Chinese languages such as Min Circle and Weining Mao, where both types of mid tone can be found. This issue is particularly interesting for any language that has more than one type of mid tone behaviour; a topic that receives all too little attention in the current literature.

B concludes with an 'Epilogue' comparing and contrasting the contour tone systems and level systems, the traditional divide between Asiatic and African languages. Although B makes some important observations, I find his generalizations about African languages to be simplistic, certainly considering the fact that only a minority of African languages actually have undergone adequate tone analysis. For example, the assertion that contours are only found on edges in word final position is simply not true for all African languages. Tonal processes do produce word medial contours in certain languages. Also the notion of tone melody over an entire word does have a certain similarity with monosyllabic contours of Asiatic languages.

In this final chapter, B presents data that are problematic to his theory. These concern syllabic nasals and obstruents, tone inventories with five contrastive level tones or more than two contrastive falling or rising tones, and finally, convex and concave tones especially those which violate the Obligatory Contour principle.

B has included a helpful appendix containing the sound systems and tonal inventories of eleven Asiatic languages.

I recommend this book for those pursuing research in tone theory in general, as well as for anyone who wants to learn more about tonal phenomena in Asiatic languages. This book could be a stepping stone to further developments for a tonal theory that is truly universal. The discussion on mid tone as well as the matter of contours, merits further reflection as it is also pertinent to certain analytical problems of some West African languages.

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Computing Natural Language. Edited by ATOCHA ALISEDA, ROB VAN GLABBEEK, and DAG WESTERSTAHL. Stanford, CA: CSLI Publications, 1998. Pp. xi, 158. Cloth $59.95, paper $22.95.

Reviewed by MICHAEL LASTUFKA
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This book contains many examples of clever sentences illustrating all kinds of linguistic problems that are difficult to solve computationally. Presented among the seven articles in the book are attempts to model the kinds of logical machinery that must be present in our ability to mentally process such sentences. Some of the articles point out difficulties, clarify important concepts or replace old methods with new approaches. Most have to do with determining the referents of words through various constraints in a particular context.

'Lexicals, contexts and unarticulated constituents' by John Perry. In the sentence, *The fish was yea big*, *yea* is an indexical: a word that refers to something that can be labeled like a pronoun but not merely a variable. Its meaning depends on context: how far apart the hands are held; where the speaker is standing; the mode of communication (i.e., face-to-face or

telephone); the subject discussed; etc.. Perry goes into some detail to distinguish the function of indexicals from other types of words that are also resolved only in context. In doing so, four categories of indexicals are demonstrated in terms of narrow and wide context and whether or not their designation is AUTOMATIC or based on the intent of the speaker.

The author gives examples of resolving indexicals at various points in cognitive parsing. Some require only syntactic information becoming AUTOMATIC or DEFAULT. Others are understood only in a local context or one referred to or implied. Still others require understanding intentions to be deduced via the gestalt of the message. As a first attempt at nailing down their function, Perry concludes that indexicals provide a way of talking about objects that doesn't require us to know much about what they are like or what their names are, but does require that we know what relation they have to us.

'Formalizing context' by John McCarthy and Sasa Buvac. 'Our object is to introduce contexts as abstract mathematical entities with properties useful in artificial intelligence. Our attitude is therefore a computer science or engineering attitude' (14). This more pragmatic approach to context seeks a minimal set of assumptions and mathematical machinery to drive them. For example, people often choose context in conversations by assuming the speaker is coherent. In AI applications, such charitable assumptions might not prove helpful. Systems built around interactive theorem provers may not be as practical as they are interesting.

Though many of the results about the truth of propositions (e.g., generalizations) in containing contexts are intuitively obvious, it is impressive that the presented framework can derive them. The framework shows mathematical concepts for entering and leaving contexts, lifting truths from one context to another, comparing truths by lifting from multiple contexts to a higher one, etc. The goal is to write AI programs with general reasoning abilities so that reasoning in a specialized context can be lifted out with meaning in more general, even transcending contexts. Examples from discourse are used.

'Changing contexts and shifting assertions' by Johan van Benthem. This paper provides a brief survey of the varied meaning of CONTEXT as used by theorists, both stated and unstated. Perry's work is compared with McCarthy's to get a clearer understanding of context as applied to logical changes in perspective. Context change shows up in standard logic in restriction, relativization, translation and preservation. It has been around for years, it just hasn't been examined explicitly for studying context. In doing so, it is shown that some of the complexities of talking about possible worlds and variable assignments in modal logics may be replaced by the notion of

context. By looking at these difficult cases as context, simpler expressions result and hopefully better understanding.

'Discourse preferences in dynamic logic' by Jan Jaspers and Megumi Kameyama. A model is constructed to deal with short discourses that have various interpretations of the action or relationships mentioned. They depend upon grammar and use of commonsense. Sentence pairs were used in a survey to determine the more likely interpretations. The clever sentences and discussion of survey results made the first part of this paper fun to read. Discussion lead to the conclusion that preference classes must exist and interact in various ways, ordered by the ability to override rival preference classes. The proposed model implements such interactions. Among topics explored rigorously by modeling were constraints for preferential dynamic reasoning and preferential dynamic disambiguation of pronouns.

'Polarity, predicates and monotonicity' by Victor Sanchez Valencia. Single words that bias the meaning of a phrase in a sentence are AFFECTIVE. In the sentence *It was dumb for Al to leave*, *dumb* places the intelligence of *Al* in question with regard to *leaving*, though intelligence was not mentioned outright. Among other properties, affective predicates allow some words to be used where they wouldn't normally appear with predicates that are related to the affective one.

In the sentence, *It was dumb for Al even to leave*, but not *It was clever for Al even to leave*, *dumb* and *clever* are on opposite ends of a spectrum; they are polar predicates. Yet the more negative word *dumb* allows the word *even* to make sense, where as *clever* does not. Because it is TRIGGERED or LICENSED in this way, *even* is called a NEGATIVE POLARITY ITEM.

This article shows that viewing affective predicates as gradable predicates, expressing position on a scale, one can prove they are logically downward monotone. This structuring allows meaning to flow from a sentence with an affective to other sentences down the spectrum to more negative meanings by use of comparatives. In order to do this, it is shown that the relevant notion of implication in monotonicity is based on comparatives giving order (gradation) to the spectrum underlying the polarity concepts.

Consider the sentences: *It was dumb for Al even to leave. Al was more sensible than Bob not to hit him back.* In these we can conclude Bob was dumber than Al for trying to start a fight. Bob is placed even lower on the scale of intelligence regarding the action than Al was. The article discusses Ladusaw's (1979) 'Licensing Hypothesis' in this light.

'HPSG as type theory' by M. Andrew Moshier. This paper is concerned with providing a way to cast Head-driven Phrase Structure Grammar (HPSG) as a type theory. By doing so, the power of type theory becomes

available to obtain more results and to take advantage of many type theory results already understood. In particular, the hope that actual parsers for these grammars can be produced boils down to building practical theorem provers for the type theory. The author concludes '...many of the familiar concerns of linguists, such as parsing, lexical rules, lexical entries, universal principles, decisions regarding "architecture" of signs, are seen to be closely related and in many instances overlapping' (138).

'Machine learning of physics word problems: A preliminary report' by Patrick Suppes, Michael Bottner and Lin Liang. The authors describe a program that solves physics problems written in English. But rather than learning about physics, it learns English, about which it initially knows nothing. An internal language represents simple kinematics (i.e., movement without consideration of forces) providing a core upon which English words can operate. Results were similar to experiences of foreign physics students who understand simple physics but not English.

The goal is to understand what kind of processes are involved in this kind of learning, not to faithfully model cognitive processes. Key notions of working memory, long-term memory, association, generalization, denotational value and memory trace were used. In particular, the program demonstrates the ability to determine which words in a problem statement are denotational—having semantic relevance to the solution of the problem. Further expected success is attributed to semantic constraints put on the input words by the associations built between them, the structured internal language and long-term memory.

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A grammar writer's cookbook. By MIRIAM BUTT, TRACY HOLLOWAY KING, MARÍA-EUGENIA NIÑO, and FRÉDÉRIQUE SEGOND. (CSLI lecture notes 95.) Stanford, CA: CSLI Publications, 1999. Pp. xi, 244. Cloth $59.95, paper $22.95.

Reviewed by TERRY MALONE
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Lately I've been scrounging around for guides to help field linguists write up their grammar analyses. The title of this book and a brief description in the back of a *Linguistic Inquiry* led me to believe that I would be reviewing another guide to grammar analysis and writing along the lines of Payne 1997 and Bickford 1998. When I opened up the book it was immediately obvious that I was wrong—or was I?

For these authors, 'writing a grammar' means producing parallel Lexical Functional Grammar (LFG) computational grammars of English, German and French. In the first part (two-thirds) of this book they present their parallel linguistic analyses of the major syntactic features of these languages and focus on the computational implications of the analyses. In the second part they discuss issues related to the computational implementation of their analyses.

The authors provide a brief introduction to LFG before launching into the description of their analyses, but I found that studying the analyses and examples in the main part of this section provided a succinct, serviceable understanding of LFG theory that extended well past their introduction. It refreshed and fleshed out what can be gleaned by wading through articles such as Bresnan & Kanerva 1989 and Bresnan & Moshi 1990.

In fact, if a field linguist wants a solid introduction to LFG theory, I would recommend first reading this section, and then using the bibliography of this book as a guide to perusing the LFG literature. The authors refer to appropriate articles for each syntactical structure that they analyze, and they also refer to alternative analyses, which makes the book an even better guide. I don't think the authors intended to produce a basic introduction to LFG—but it is a happy and useful side-effect of their work.

The authors did, however, quite deliberately construct parallel grammars of three related languages, with the intention of limiting possible analyses for any given grammatical structure. The idea was, where possible, to choose one single analysis for a given grammatical category that could be implemented in all three languages. One result is a fairly thorough review of all significant syntactic features in the three languages. Although the authors are thinking more in terms of a guide to producing computational grammars,

their syntactic review is a handy guide to the minimum of syntactic features which should be included in any non-computational grammar.

The unique approach in this section results in several fascinating analyses. Perhaps the most intriguing is the joint analysis of the auxiliary systems of the three languages. There are two ways to approach auxiliaries: they may be treated as raising verbs; or they can be treated as merely feature-bearing elements which do not subcategorize for other elements (i.e. have grammatical dependents). The authors chose the latter analysis, finding that within the confines of LFG grammar it allowed them to produce a computationally unified analysis for all three languages.

The computational success of the analysis (and others proposed in the book) hinges on LFG's separation between syntactic structure and function. The authors note repeatedly that syntactic functions ('f-structure') of syntactic elements were very similar or identical for all three languages, and that language specific idiosyncrasies could be relegated to the structural description (a tree diagram, or 'c-structure'). This approach turns out to be especially effective for elements such as auxiliary verbs, which exhibit idiosyncrasies in each of the three languages under consideration.

Field linguists, who usually have to deal with more inflection than English, French and German offer, will no doubt want to know whether and how the authors incorporated morphology into their grammar. Fortunately (as they note), German has enough morphology that they had to face this issue. LFG handles inflectional morphology by means of 'm-structures' (specifications of 'wellformedness conditions' for words in a given position in the 'c-structure'). Unfortunately, very few examples of m-structure specifications are found in the text (see p. 65 for the only example I could find), but the authors do refer the reader to literature which discusses m-structure in more depth.

Field linguists who have to deal with languages in which pragmatic considerations control word order will want to know how the grammar writers handled variable word order in German. The authors offer even less detail than they did for morphology, but they again fill in this gap by referring the reader to appropriate LFG literature.

The second part of the book, 'Grammar engineering', was no less fascinating to me than the first part, perhaps because in the early 1980s I was linguistic consultant for a computer assisted dialect adaptation project. Most of the problems which the authors encountered in their computational implication of the grammars were familiar—two programmers and I had thrashed

through many of them nearly twenty years before.[1] I even found myself wishing I could stuff this little book into a time machine and send it back to those days. The three of us could have benefited from the simple, clear description in this section.

It is so well written that I would recommend it to any linguist struggling with parsing programs, a computer assisted related-language adaptation program such as CARLA, or the complexity of the LinguaLinks morphological analyzer. It discusses in a fair amount of detail basic considerations controlling the design of this book's tri-language grammar, and most of these same considerations also control the design structure of parsing and adaptation programs.

For example, in this section the authors demonstrate statistically that maintaining multiple parsing dictionaries significantly cuts the time it takes the computer to parse language text. Somehow this detail escaped me in both AMPLE (A Morphological Parser for Linguistic Exploration) and CARLA documentation (Weber et al. 1988 and Buseman 1993). I don't know about other linguists, but knowing the logic behind the design helps me use computational tools more intelligently, and is helpful to my less than abundant patience when things don't work out right the first time.

Another device which cuts down on parsing time and possible analyses for certain problematic structures is the incorporation of rudimentary Optimality Theory (OT) into the program design.[2] Linguists working within mainstream Chomskyan syntax have been trying to incorporate OT into syntactic theory with variable results (see Pesetsky 1997 and Speas 1997 for two distinct points of view), so it was a surprise to find that LFG practitioners are successfully incorporating it into their theoretical approach. The authors quite nicely demonstrate the computational advantages of incorporating optimality ranking for possible multiple parses.[3] . The computational version that the authors use, however, is much more limited than what is envisioned in phonological versions.

After finishing this book, I went back to an older introduction to computational linguistics (Grishman 1986) in order to check out several computational terms, and immediately discovered another happy side-effect of reading *The Grammar Cookbook*: Grishman's book made more sense than

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[1] See Reed 1985 for a summary of our agonies, and compare it to the second part of this book.

[2] See Cahill 1999 for an overview of Optimality Theory.

[3] They cite several examples. One is 'The [print quality] of this printer is good' and 'I want to [print] [quality] documents'. Here '[print quality]' must be parsed as a technical term, and '[print] [quality]' must be parsed as a verb followed by part of its dependent.

it had fifteen years ago. I don't think this was the author's fault—a computer programmer friend who was getting into linguistics thought the world of Grishman's tome. But it demonstrates that in spite of involvement in a language adaptation project years ago, and the abortive attempt at reading Grishman, I had to read *The Grammar Cookbook* in order to acquire an adequate feeling for what computational linguistics is, and what it can do for me as a field linguist and an end-user of parsing programs.

I only found four typos in the book, two in German examples (22, 135) and two in the text (72,196). The typo on p. 22 could affect the reader's comprehension of the point being illustrated; the example should read *Den Traktor startet der Fahrer.*

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Survey of the state of the art in human language technology. Edited by RONALD COLE, JOSEPH MARIANI, HANS USZKOREIT, GIOVANNI BATTISTA VARILE, ANNIE ZAENEN, ANTONIO ZAMPOLLI, and VICTOR ZUE. (Linguistica cumputazionale, Volumes XII-XIII,) Pisa: Giardini; and (Studies in natural language processing,) Cambridge: Cambridge University Press, 1997. Pp. xx, 513. Cloth $49.95.

Reviewed by MIKE MAXWELL
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'Human language technology' is used here to mean computational linguistics (more or less; I will come to the 'less' in a moment). The domains covered are:

- Spoken language input (speech recognition and understanding; speaker recognition)
- Written language input (converting printed or handwritten copy into computer-processable form)
- Language analysis and understanding (grammar formalisms, lexicons, parsing and semantic analysis)
- Language generation (producing text from non-text data)
- Spoken language output
- Discourse and dialogue (particularly as these relate to text understanding and production, and to user interaction)
- Document processing (retrieval and summarization of computer-processed documents, as well as tools to help people create documents)
- Multilinguality (machine translation, and information retrieval in a multilingual environment)
- Multimodality (integrating text representations with visual representations, using gesture together with speech; improving speech synthesis and recognition using visual cues)
- Transmission and storage (speech processing for noisy environments and limited bandwidth channels)
- Mathematical methods (statistical modeling, digital signal processing, parsing, connectionism, finite state grammars, and optimization)
- Language resources (corpora and machine-readable lexicons)
- Evaluation (how language software can be evaluated against its task, and how different implementations of language technology can be compared among themselves)

Each field is introduced by a leading researcher, then other investigators are given several pages each to discuss their areas of expertise within each topic.

Not every area of computational linguistics is well covered. The 'Forward by the Managing Editors' includes the following (page xvi):

> Languages for which no adequate computer processing is being developed, risk gradually losing their place in the global Information Society, or even disappearing, together with the cultures they embody, to the detriment of one of humanity's great assets: its cultural diversity.

Nevertheless, the text itself has virtually nothing to say about tools for any but the largest languages. The chapter on 'Language resources', for example, discusses available corpora and dictionaries in major languages, but the question of how such resources might be compiled for lesser-studied languages does not come up. Indeed, the entire area of field linguistics, including computational tools for doing field research, is conspicuous by its absence. In light of the above statement by the editors, it seems that at least a passing mention of work with smaller language groups would have been appropriate.[1]

Computational phonology (the subject of a special issue of the journal *Computational Linguistics* and of several workshops sponsored by the Association for Computational Linguistics) is another area not addressed; the index cites only a handful of pages on phonology, most of which turn out to be irrelevant. While computational phonology has admittedly not been important to most commercial efforts (even in speech technology), it is a part of 'human language technology', and may yet prove its worth.

A third area not covered is that of machine translation among closely related languages.[2] Between closely related languages, the mapping between the world and the language is often reasonably parallel, and the requirement for translation arises largely from lower level phonological, morphological, and (simple) syntactic differences, combined with pride in one's own language. The issues of widely divergent lexica and grammars, and the resulting need for actual text UNDERSTANDING—issues which wreak havoc with machine translation between languages like French and English—are much ameliorated, so that word-for-word and even (to some extent) morpheme-for-morpheme translation becomes feasible. For example, morpheme- and word-based translation of legal documents has proven useful between the

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[1] There are a number of groups working on the development of computer resources for minority languages, among them the Department of Linguistics at Lancaster University and Oxford University Computing Services, with a joint project called 'MILLE' (Minority Language Engineering), the Computing Research Laboratory at New Mexico State University, with the Corelli project; and of course SIL International. There are doubtless many other efforts.

[2] Commonly known by the acronym CARLA: Computer Assisted Related Language Adaptation.

closely related languages Catalan and Aranese Occitan (Tim Erickson, personal communication).

Finally, the question of non-Roman scripts is covered in a couple short paragraphs—enough to note that there are problems, but not much else. As recently as 1989, a strike on the part of calligraphers in New Delhi stopped Urdu language newspapers from being printed. There were at that time no adequate typesetting facilities for the script, and newspapers went from hand-written copy to photocomposition. Computer typesetting of Urdu has improved since then, but this is clearly an important area of language technology.

As for the topics that are covered more thoroughly—the bulk of language technology, to be sure—the depth of coverage is something more than an annotated bibliography, and something less than a handbook. For example, the section on computational morphology takes up about two pages (96-98), enough to mention the main technologies and cite the important references, but not enough to really explain how these technologies work or what their advantages and disadvantages are. The following discussion of two approaches to computational morphology is not atypical (97-98):

> ...the entire lemma can be computed in advance and stored as a finite-state transducer whose arcs are labeled by a pair of forms... The number of nodes in this type of network is small, but the number of arc-label pairs is very large as there is one symbol for each morpheme-allomorph pair.
>
> A more optimal lexical transducer can be developed by constructing a finite-state network of lexical forms, augmented with inflectional tags, and composing it with a set of rule transducers [references omitted—MM]. The arcs of the network are labeled by a pair of individual symbols rather than a pair of forms. Each path through the network represents a lemma.

For the reader unfamiliar with what is being described here, it might appear that the 'more optimal lexical transducer' would always be preferable. But this is really true only if (1) the language being analyzed has a good deal of morphology, (2) phonologically predictable allomorphy is widespread, particularly in open-class items (stems, not just affixes), and (3) allomorphy is distinguished in the writing system. English is a good candidate for a language in which none of these criteria is met. For example, the allomorphic alternation between /s/ and /z/ in the noun plural suffix is never represented in writing, and the alternation in the same suffix between /z/ and

/əz/ is not consistently represented.[3] The reader may also wonder what kind of rules the 'rule transducers' handle, what 'inflectional tags' are, and just what the distinction is between 'individual symbols' and 'forms'.[4]

In summary, the broad nature of this book results in a fairly shallow, and at times confusing, coverage. At the same time, the volume already comes to more than 500 pages, and one suspects that a true handbook surveying the entire field of computational linguistics would have been an impossible dream. Seen in that light, it is a reasonably useful work.

Computational linguistics is an ever-changing field, and one must ask how fast this book will be outdated. It was published in 1997 (and this review is being written in 1999). But the articles appear to have been written no later than 1994 (one section is entitled '...systems available in 1994'; the latest references date from 1994, with a handful of citations for 1995). Where it touches on hardware and on commercial software, it was outdated even before publication. The article by Christian Boitet on machine translation repeatedly refers to Microsoft Windows 3.1 and Microsoft Word 6, both of which were superceded before the publication date, and have been superceded once or twice more since then. Many of Boitet's points are still valid, but could have been made more convincingly by omitting reference to specific versions of software.

At the same time, some things have not changed recently—and have never changed, apparently. In another article on machine translation (248), Martin Kay writes:

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[3] An example of the inconsistent representation of the /ɪ/-/ɔɪ/ alternation is *place-places*, where the *e* in the second word does double duty for indicating 'length' on the stem vowel and for the epenthetic schwa.

[4] The larger text from which the quotation is taken refers to both 'cut-and-paste rules' and 'rules for morphological alternations.' The 'rule transducers' in the quotation treat the latter sort of rules, i.e. (morpho-)phonological rules. Spelling rules can be handled in much the same way, but are not discussed. English is in fact a good candidate for the 'more optimal lexical transducer'—not because of allomorphy, but because of predictable spelling alternations (like *try-tries*). But this is not pointed out in the text. (The authors of this section are well aware of the usefulness of treating spelling alternations in this way; perhaps the omission arose during editing.) As for the remaining terms, the 'inflectional tags' refer to morphosyntactic properties such as category (part of speech) and inflectional features. 'Form' refers to the phonological (or orthographic) forms of entire words and morphemes, while 'symbol' refers to the individual phonemes (or letters, in a orthographic system) of which those 'forms' are composed. The difference between the two kinds of lexical transducers is similar to the difference between Item-and-Arrangement morphology and Item-and-Process morphology (in which phonological rules modify underlying forms to produce surface forms).

> The field of machine translation has changed remarkably little since its earliest days in the fifties. The issues that divided researchers then remain the principal bones of contention today.

A remarkable statement! Kay of course goes on to highlight some of the changes which have taken place, although his point remains valid.

In short, fundamental issues change slowly; implementations change so quickly that any description is likely to be out-of-date as soon as it is written. The editors hint that there will be 'updated and improved versions of this work' (xviii). It may be that the only way to keep a work of this sort current is to publish it on-line.[5] Perhaps at the same time, it should be recast as a handbook of fundamental topics of human language technology, with references to newer developments added regularly. Discussion of the latest implementations, on the other hand, might best be handled by newsletters or such.

There are very few illustrations, and a few of the illustrations which do appear are chart junk which should have been omitted. The volume is well-edited, apart from the occasional odd wording by a non-native speaker of English. The indexing appears to be machine-generated, with some human post-editing. This results in a few peculiarities.[6]

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[5] As this review is being written (August 1999), the book is available in HTML and Postscript form on the Internet at the following URLs:
http://www.dur.ac.uk/~dcs0www3/lnle/survey/HLTsurvey.html,
http://www.elsnet.org/publications/hlt/, and http://cslu.cse.ogi.edu/HLTsurvey/HLTsurvey.html.

[6] The irrelevant references to phonology were mentioned earlier. (Actually, there is no entry for 'phonology', but there is an entry for 'phonological'.) There is a single page cited for 'linguists', which reports linguists' historical attitude toward finite state grammars. There are two separate index entries for 'finite state' and 'finite-state', distinguished only by the hyphen. There is an entry for 'user' with a subentry for 'model', but no subentry for 'user' under the entry for 'model'

Intonational phonology. By D. ROBERT LADD. (Cambridge studies in linguistics 79.) Cambridge: Cambridge University Press, 1996. Pp. 334. Cloth $23.95.

Reviewed by KEITH SNIDER
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Intonational Phonology, written by one of the leading experts on linguistic intonation, provides the best overview in existence on the subject of intonation and will not be demoted from that position any time soon. Given that all languages employ intonation phenomena, this work should have broad appeal, and anyone with a serious interest in analyzing the intonation structures of a language will find it invaluable.

In the introduction, Ladd states his belief that intonation has 'a phonological organisation' (1), despite the fact that intonation is often used to signal paralinguistic information. His stated goals (3-4) for the book are:

- to 'set out the theoretical assumptions and empirical foundations of the general autosegmental-metrical approach to intonational phonology';
- to 'demonstrate the potential of the theory'; and
- to 'state the case for my own particular version of the general autosegmental-metrical theory'. Each of these goals is achieved with excellence.

Here are the contents of the volume:

Introduction
1. Introduction to intonational phonology
2. Fundamental concepts of the autosegmental-metrical theory
3. Phonological representation of pitch in the autosegmental-metrical theory
4. Cross-language comparison of intonation
5. Patterns of prominence
6. Prosodic structure
7. Pitch range

L defines intonation in Ch. 1 as 'the use of SUPRASEGMENTAL phonetic features to convey "postlexical" or SENTENCE-LEVEL pragmatic meanings in a LINGUISTICALLY STRUCTURED way'. In this chapter he outlines the history of intonational research, focusing on helping the reader to distinguish between, on the one hand treating all intonation as paralinguistic, and thus a priori ruling out the possibility of finding phonological structure in intonation, and on the other hand treating all intonation as phonological. As an Africanist, I found particularly relevant his claim that the lexical tone of tonal languages is not merely an overlay on the phrasal or global intonation

melodies of those languages. Rather, L argues convincingly that intonation should be treated as boundary tone phenomena. For in depth treatments of boundary tone phenomena in tonal languages, I refer the interested reader to Hyman 1990 and Black 1995.

In Ch. 2, L explains the fundamental concepts of the autosegmental-metrical (AM) theory. Again, as an Africanist, I didn't have a hard time believing that pitch phenomena could be handled insightfully with autosegmental theory, but before reading the book I was unconvinced that we really needed metrical theory. However, L argues that in some languages certain facts of intonation can ONLY be explained by assuming that some syllables are metrically strong. He demonstrates this by showing that in French even though final syllables are usually weak phonetically, surface intonation melodies are best explained by assuming that certain final syllables are metrically strong.

L describes Pierrehumbert's (1980) model of intonation in depth in Ch. 3 and in his introduction considers it to be the 'single most influential contribution to current work on intonational phonology' (3). He discusses two problems with her model—her account of phrase accent, and her account of downstep, and concludes that these can be better handled another way. Given my own criticisms of Pierrehumbert's account of downstep (Snider 1998, 1999) I would agree with him.

In Ch. 4, L makes a very interesting cross-language comparison of intonation phenomena. Phenomena generally associated with intonation are: declination, low or falling pitch, and high or rising pitch. I noted with interest that he differentiated tonal languages from purely intonational languages by their 'core' tones. Tonal languages have lexical core tones, while purely intonational languages have pitch accents that go to prominent places in the phrase.

Ch. 5 discusses patterns of prominence, that is, the location of pitch accents. L shows that 'in their melodic features, languages differ systematically in their patterns of utterance-level accentuation' (160).

In Ch. 6, L discusses prosodic structure. He correctly notes that to date the majority of work on intonation has been mostly autosegmental in nature. In this chapter he sets out his own version of how metrical theory can contribute positively to the study of intonation. He addresses three main issues: the internal structure of tunes, the phonological nature of sentence stress, and the nature of prosodic constituency.

The final chapter, Ch. 7, deals with the subject of pitch range. I was intrigued to discover that not only do speakers manipulate their pitch range, i.e., the total space between the minimum and maximum pitches used in normal

speech, by moving the range up and down, but also by EXPANDING it, i.e., by simultaneously raising the upper end pitch of the range used in normal speech AND lowering the lower end pitch of the range used in normal speech.

The physical attributes of the volume are good. I didn't notice any typos or misprints, the print size is adequate, and the binding is of high quality. I was, however, disappointed that the publisher chose to use end notes instead of footnotes. I always find end notes frustrating.

Anyone with a serious interest in analyzing the intonation structures of a language will find this book invaluable. It is not only well thought out, but also well written. Having said that, the prospective reader should bear in mind that the work is quite theoretical and definitely not bathtub reading. Nevertheless, L's many years of experience bring to the volume a richness and depth that make the effort invested in reading it well worthwhile.

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Eve spoke: Human language and human evolution. By PHILIP LIEBERMAN. New York: W. W. Norton and Company, 1998. Pp. 192. Cloth $25.00.

Reviewed by THOMAS M. TEHAN
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This is a book you might easily find yourself reading in a weekend. Its scope and breadth of material is broad. Lieberman strives to integrate material ranging from the characteristics of a reconstructed Neanderthal's vocal tract to detailed data on human speech deterioration during a climb of Mt. Everest, from chimpanzee speech experiments to aphasia in Parkinson's

disease, and also the basics of human brain anatomy as it relates to speech. He discusses the role of chimpanzee culture in tool-making, chimpanzees' articulatory abilities, and even describes chinchillas' (rodents) ability to distinguish /p/ and /b/. He also relates facts from acoustic phonetics.

Eve Spoke is written at a layman's level, without specialized linguistic vocabulary; for instance, L explains how the vocal cords generate sound. He also discusses in very accessible language how 'the human speech perception system operates in terms of...longer syllable-size chunks of speech, which obviously occur at a lower rate than the individual phonemes' (14).

L states (xiii): 'This book is about how we came to be; part of the answer is that speech and language shaped the evolution of our immediate ancestors, the first modern human beings.' L assumes that there was 'a common ancestral language' from which modern languages have diverged syntactically and morphologically (10).

Without actually making the case for the evolution of language, L uses many insightful descriptions of present day systems to help his readers imagine what a developmental process of language evolution might have been like. If humans have evolved from non-speaking predecessors, speech must have evolved from non-speech sound systems, using vocal tracts and brains not originally adapted for speech.

The 'language-thinking system' which 'entailed the restructuring of anatomy originally adapted for eating, breathing, and making a limited number of sounds and modifications to the brain (which are still not understood), involved the same Darwinian mechanisms that produced the distinctive attributes' (xiv).

L asks: 'From where do both the similarities and differences between human beings and animals come?' His theory is that evolutionary processes produced the difference from the same ancestors. He does not consider that a designer may have created both similarities and differences.

Ch. 1, 'The mice talked at night' (a quote from Beatrix Potter), illustrates the complexity of the speech perception-production process. He concludes the ch. with 'Darwin's Solution', in which he personifies evolution, giving it decision-making abilities and personality:

> In simple terms, evolution is miserly and opportunistic. The goal is to achieve a result by spending as little as possible and making do with what you already have. (18)
>
> The complex human speech system is a typical case of evolutionary tinkering. We have retained the general-purpose 'primitive' mammalian auditory system and simply added a special-purpose speech-encoding system that handles a

> special function.... A second system, which has as its DESIGN OBJECTIVE contrast enhancement, has been added to the older, primitive system. If we were LOGICALLY constructed, it would have been more ECONOMICAL simply to replace the old system.' (19, emphasis mine—TT.)

L envisions 'evolution TINKERING with devices that originally were used for other purposes ... originally DESIGNED for swallowing food and breathing. They were modified so that we could produce sounds that were easier to understand' (20, emphasis mine—TT)

Ch. 2, 'Chimpanzees and time machines', covers chimpanzee behavior, culture (including tool making and warfare), language (spoken or signed) and speech anatomy. L concludes that distant evolutionary hominid ancestors 'undoubtedly' had many of the same characteristics as chimpanzees (48).

Ch. 3, 'He's a big baby', describes Neanderthal speech capabilities and culture inferred from skeletal remains and artifacts. Surprisingly, the vocal tracts of newborn humans, monkeys and apes are similar to the Neanderthal's, whose vocal tract differs profoundly from that of normal adult humans (51). L asserts that Neanderthals could not have said the vowels [i], [u] and [a] and the consonants [k] and [g] as clearly as we (65).

> 'However, these speech deficiencies do NOT mean that Neanderthals did not talk or that they did not possess language.... Neanderthal speech-producing anatomy [brain included] could have produced almost all of the speech sounds that occur in human languages' (66).

Ch. 4, 'Dead men and women talk again', uses the prevalent model of hominid evolution to imagine the speech capabilities:

- of the 2-million-old australopithecines whose vocal tract was 'virtually identical' to those of present-day chimpanzees (86);
- of the 1.5-million-old Homo erectus who 'clearly possessed manual, cognitive and probably language abilities commensurate with increased brain size' (80);
- of the 200,000-year-old Neanderthals whose speech anatomy was more advanced (95);
- and of modern human beings, the progeny of Eve and Adam, who 'prevailed because they talked' (xiii, cf. also p. 68).

L believes that 'our ability to talk is one of the keys to understanding the evolutionary process that made us human' (xiii).

Ch. 5, 'Talking and thinking brains', discusses findings in research relating brain anatomy to speech functions. However, little is done with the

information to establish that this has anything to do with the evolution of human speech other than imagining that as the brain size of evolving hominids increased, so did its complexity.

In the final ch. 'What, when and where did Eve speak to Adam and he to her?', L concludes (133):

> The answers to the questions in the chapter title all hinge on which Adam and Eve you're thinking of. In the five-million-year-long lineage that connects us to the common ancestors of apes and human beings, there have been many Adams and many Eves.... In the beginning there was the word, but the vocal communications of our most distant hominid ancestors five million years or so ago probably didn't really differ from those of the ape-hominid ancestor. Most of their cries would have been linked to emotion and instinct. ... the ability to produce vocalizations that are NOT linked to emotion and instinct seems to create the gulf between human language and the vocal communications of apes.

Ten pages of endnotes are included, as well as a 16-page bibliography and 13 pages of useful Index.

Recorded history takes us back little more than 5000 years. Widely accepted historical linguistic reconstructions reach no farther back. L admits that 'Inferences concerning prehistory, like other theories, are really informed guesses that are then tested against scientific evidence' (xiv). L proposes a model for how language might have evolved based on research in present day capabilities of humans and animals, and on reconstructions from fossils using their assumed age. This being the case, his language evolution theory remains an unverifiable historical model.

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Ideology in the language of judges: How judges practice law, politics, and courtroom control. By SUSAN U. PHILIPS. Oxford: Oxford University Press, 1998. Pp. 123. Cloth $59.00, paper $29.95.

Reviewed by BARBARA THOMAS
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Ideology in the Language of the Judges gives evidence that people do not compartmentalize their beliefs, however much they may wish to or believe that they are capable of it. Ideology reveals itself in behavior and speech. Susan Philips is an anthropologist who decided to study culture and social structure in the U.S. court system. To acquaint herself with the 'language'

she attended a year of law school. She writes about her research of nine judges' discourses in the Pima County Superior Court of Arizona, specifically, in how they propagate ideology by the way they hear guilty pleas. As she unfolds her research results, she also builds a case that certain beliefs about the justice system are myths:

Myth #1, The written and oral law is one. In actuality, the written law sets boundaries on what is not acceptable in following due process of law. This leaves considerable room for interpretation on what is necessary for compliance. Beliefs about what is necessary to insure compliance with the law will be based on a person's value system (people vs. tasks) and political worldview (rights of people vs. rights of state).

Myth #2, Politics does not enter the judgeship. The research in this book is focused on trial court judges who are deciders of fact. (Appellate court judges are deciders of law and it is accepted that they would be influenced by their political beliefs.) One would think that having appointed rather than elected judges would indeed divorce politics from the court system. Philips interviewed four appointed judges and found that though they distanced themselves from politics, they planned their careers to please those who prepared the short-list of possible appointments to the judgeship and felt that they received their appointment because they were close friends with someone who was active in politics. Those judges who were appointed felt more closely bound to the written law than did their elected counterparts.

Myth #3, Courtroom order is independent of political ideology. Philips explains that in the law concerning the guilty plea, the judge is called upon to make sure that the waiver of rights was explained to the defendant and that the defendant waives those rights willingly and knowingly. The law does not state that the judge must inform the defendant, only that he/she should make sure someone has done this. Yet in this procedure, a dichotomy emerges between what Philips terms 'procedure-oriented judges' and 'record-oriented judges'. Procedure-oriented judges are more liberal and tend to see themselves as protectors of the individual, who need help from the state to obtain due process of law. They take on the task of informing the defendant of his rights and waiver to those rights. They involve the defendant, take more time, vary the way they proceed with different defendants, and risk losing courtroom control because they do involve the defendant. (The defendant's answers, typically denying a certain crime, being ambiguous or adding mitigation, could legally invalidate the plea and require the judge to regain verbal control so that the guilty plea can continue.) Procedure-oriented judges desire the defendant to see them as friendly and want the defendant to feel comfortable and free to speak in 'the people's' courtroom. However, courtroom control is one way in which

judges are evaluated by the bar. To lose control of the courtroom is to risk one's reputation among one's peers as well.

Record-oriented judges adopt a more conservative view of the relationship between a person and the state. They do not personally assume responsibility but do ascertain that someone has informed the defendant of his/her rights. Responsibility for informing the defendant of his rights is shared between the defendant, lawyers and judge, but the record-oriented judge assumes the defendant knows what he is doing. They do not invite much involvement of the defendant. They tend to ask yes/no questions of the defendant rather than WH-questions. They tend to follow the same procedure from case to case so as not to miss points of the law and give the defendant some credit by assuming his/her ability to understand what they are told. They see courtroom control as important and view procedure-oriented judges as inviting rebellion and disorder in the courtroom.

Philips takes issue with the predominant form of functionalism currently taught in linguistic anthropology: form is multifunctional, but only one form is realized in an instance and is determined by the context. Philips argues that judicial behavior acts out more than one belief framework, carries more than one meaning and what is meant is not determined by the context of the behavior. The way judges conduct guilty pleas is the sum of their beliefs in politics, interpretation of the written law, and courtroom control. Hence, Philips concludes that trial lawyers really do practice politics and exercise power.

Philips' text is only 123 pages long, but I found her verbose sentences tedious, at times getting lost in her arguments and not being with her by the time she stated her proofs. Her social background questionnaire and quantitative methods could aid a field anthropologist who wishes to study ways in which power groups are erected in society. I can accept that our actions are the sum of our beliefs, however, I am not convinced that there is the dichotomy that Philips would have us believe—a struggle between the people and the state. In spite of her arguments, I suspect there is more of a spectrum reflecting a continuum of beliefs in a world full of diverse people. Still, it is refreshing for a field linguist to see anthropological and linguistic studies taken to the city.

For those interested in beginning or furthering their studies in the use of language in the legal system, there is a bibliography, Levi 1982, which covers a wide range of topics with an easy-to-follow topic outline summary

at the beginning. She also gives beginners direction by suggesting they first read certain articles and books.

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Historical linguistics: An introduction. By LYLE CAMPBELL. Cambridge, MA: MIT Press. 1999. (Published in 1998 by Edinburgh University Press.) Pp. 396. Paper $30.00.

Reviewed by PETE UNSETH
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Campbell's new historical linguistics textbook is an excellent tool. As a first introduction to historical linguistics, the book is well planned and the material carefully sequenced. Exercises are included at the end of each chapter. The steps of comparison and reconstruction are clearly laid out. The pages are of excellent quality paper, though the cover stock is too thin. At $30.00 US, the price is reasonable. For those, like myself, who must study historical linguistics independently, using a manual rather than in a class, the book is complete enough to be used by itself.

The book is accessible to readers of varieties of English. It is written by an American, but published first in Britain. Written with a broad audience in mind, it has synonyms given for both North American and British, e.g. *sledge* and *sleigh* (347).

As always, each reviewer will have some points that they wish the author had added or expanded upon. For example, I was disappointed that C did not include serious discussion of word order changes (i.e. in sections 3.7 or 9.2.3), the pattern of archaisms preserved in peripheries, pidginization and creolization, reconstruction of affixes (not just roots). Potentially confusing typos are rare: e.g. *elits* for *elpits* (214.

C's book is distinguished from other historical linguistics texts by two major points. First, in historical linguistics, readers of historical linguistics texts are traditionally shown examples from Germanic and Romance languages, Finno-Ugric (thanks to Anttila and Lehiste), plus other Indo-European

languages. C reaches out beyond these to illustrate the principles of historical linguistics and presents exercises with data from less familiar languages, especially Mayan, and Mixean languages. He is especially innovative in the area of philology, (working with old written records of a language), using Mayan data. But in this case, less exotic writing systems might be more readily understood by the novice.

C's second significant difference from previous texts by other authors is his discussion of work in long range linguistic comparisons, though always negative in tone. There has been a flurry of scholarly activity in the last 20 years in comparing languages that are only distantly related, if at all, e.g. Athabaskan (of North America) with Sino-Tibetan. Some of these proposed relationships, such as Afroasiatic, are actually widely accepted, despite C's skepticism. Others are not taken seriously by even a handful and these cases are an embarrassment to those that are more conscientious. C tends to lump all these broader comparisons together, attributing the errors of some to the broader group of scholars.

These long-range comparisons have generally been based on putative shared vocabulary. C's opposition to long range comparison is rooted in his adherence to a careful methodology based on demonstrable sound changes and other shared innovations. For example, comparativists should not use contemporary forms in comparisons when the reconstructed ancestral form is not as similar. As an example of this, an author had compared Koasati *bit* 'dance' with Mayan *bis* 'dance' but since Koasati /b/ is derived from Proto-Muskogean *k^{w}, the actual similarity is not as close as the author had portrayed it.

In demanding evidence of sound changes, C sets a very high standard for demonstrating relationship between language families. Though C may be setting a standard that is unattainable at present, (given the state of our knowledge of many language families), it would be very surprising if future scholars, with more language data and an expanded body of comparative studies, were not able confirm some of the postulated links between some of the families that C currently sees as yet unrelated, especially several families in the Americas. Given the large number of language families in close proximity in the Americas, is it not to be expected that some of these families share a common origin?

His chapters on semantic change and areal linguistics and his sections on grammaticalization and naturalness of certain changes (phonological and semantic) are valuable, topics not covered in every textbook. He also discussed the history of the development of historical linguistics, using the different perspectives to illustrate different insights.

Like every historical linguistics text, C struggled with how to represent the forms under discussion: whether to give them in their orthography, in phonemic form, or phonetic form. The result is a low level of confusion; for example, readers find *y* at times representing a vowel, other times a consonant.

Some field linguists may see historical linguistics as irrelevant and difficult, possibly under the misimpression that it is concerned only with the study of dead or ancient languages. My study of historical linguistics enabled me to rationalize patterns in Amharic language school and to understand classes of irregular verbs in the Majang language of Ethiopia on which I had done field research. It also equipped me to glean additional insights by reading the descriptions of Majang's sister languages, and in general, has provided me endless fascination.

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Books Available for Review

The following books are available for review by *NOLx* readers. The reviewer keeps the book in exchange for submitting a publishable review. Contact by email <mike_cahill@sil.org> or snailmail: Notes on Linguistics, 7500 W. Camp Wisdom Road, Dallas, TX 75236 USA.

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FROM THE LINGUISTICS COORDINATOR

Coaching and Cheering

In my first column as International Linguistics Coordinator, I listed six areas of services we in the International Linguistics Department want to provide to our SIL members on the field. These were Caring, Communicating, Coaching, Cheering, Check-up, and Cultivating the Vision. In this issue, I'd like to focus on the middle two of these, Coaching and Cheering, since these are especially timely right now. I hope to talk about the others in the next few issues.

Coaching: We in the International Linguistics Department want to help you do the best linguistics you can. In pursuit of this, we do a fair amount of unseen work, such as occasionally advising SIL schools on courses and working with others to help produce linguistics software, guidelines and helps. Some of these helps eventually become more visible, such as the books and modules in the Linguistics section of the LinguaLinks Library (if you haven't got a copy of this CD yet, get it! It is literally a library of resources on not only linguistics, but language-learning, literacy, anthropology, sociolinguistics, Scripture use, even a couple cookbooks and medical references). Of course, we have also done a bit of coaching through 'how-to' types of articles in Notes on Linguistics in the past, and you can look for more of these in the future.

The most obvious 'coaching' is of course consulting, whether in workshops or one-on-one. When I ask entities how we can help, the usual answer is 'Send us more consultants!' I really wish we had a building full to send! We need to grow more of them, both in the local entities and at the international level. One concrete action we've taken in the last two years is to run two workshops in Dallas to train and equip more linguistics consultants. This year, we're putting a different twist on this and will specifically train discourse consultants in Dallas this fall.

Finally, if you are passing through Dallas and need help with a linguistics problem, we'll do our best to connect you with someone here to help.

Cheering: We want to recognize significant achievements in linguistics. One way we have done this in the past is by printing Ph.D. dissertation abstracts in Notes on Linguistics. We will continue to do this. But

dictionaries and in-depth grammar write-ups are equally deserving of recognition. I try to keep abreast of such things, but still depend on you to keep us informed.

If you have any ideas for further recognition, please let me know. I would love to hear of how local SIL entities are recognizing linguistics production. We don't want to promote pride and swelled heads, but we do want to acknowledge the significance of linguistics in our SIL work.

A final and most heartfelt cheer is due to David Payne, who has served as the editor for Notes on Linguistics since 1994. During the first few years he was International Linguistics Coordinator. But for the last four years he has continued his editorship and thus taken a significant load off his successors as Coordinator, first Lou Hohulin and now me. David has continued editing in the midst of being involved in two New Testament translations simultaneously in Peru, making many trips between the USA and Peru. At this point in his career, David will be moving forward into new responsibilities which will consume even more of his time, and will relinquish the editorship of Notes on Linguistics as of this issue. Until and unless another editor springs forth to help, I will be serving as editor of Notes on Linguistics. I know I echo the sentiments of many when I say a deep and sincere 'Thank you, David! We have appreciated the diligence and skill you bring to your work. Blessings to you in your new roles.'

—Michael Cahill
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■

REVIEW ARTICLE

The Amazonian Languages. Edited by R.M.W. DIXON and ALEXANDRA Y. AIKHENVALD. 1999. Cambridge & New York: Cambridge University Press. Pp. xxviii, 430. Cloth $69.95.

Reviewed by TERRY MALONE
SIL—Colombia

0. Introduction. Until recently modern linguistic theory, as practiced in the U.S. since the appearance of Chomsky's well-known 1957 and 1965 books, has taken little or no note of Amazonian languages. Two distinguished linguistic scholars have commented on this situation: 'When we began working together ... in the late 1970s ... there were scattered individuals in a number of countries in South America, mostly members of Christian missions, who were studying individual Amazonian languages, but general linguistics was being practiced almost entirely without reference to even the existence of Amazonian languages' (Derbyshire and Pullum 1998:3). This was in spite of sporadic attempts to challenge theory on the basis of data from Amazonian languages (for just a few examples see Pike and Kindberg 1956, Pike 1964, and David Payne 1974). Most, but not all, of this work tended to appear in out of the way publications, theses, dissertations, and specialist journals ignored by the theoretical mainstream.[1]

To be sure, some work in generative phonology had already begun to take notice of languages in the Amazonian area. The work was mostly related to developments in metrical theory (several works of Bruce Hayes), or in specific languages (work such as Everett and Everett 1984a, b). Earlier work in autosegmental theory, chiefly with respect to nasal prosody within the more general context of harmony systems, had taken a passing glance at languages of the region (see, for instance, Safir 1982, van der Hulst and Smith 1982 and Hyman 1982). These developments inspired theoretical linguists to dust off works such as Bendor-Samuel (1960, 1966), and Kaye (1971), or scrounge around for work such as David Payne (1974) and Smith and Smith (1971),[2] but it was developments in typological theory, in part as a

[1]The mainstream has equally overlooked the work of non-missionary linguists. One good example is Kaye (1970), who in spite of a short time in the field managed to produce a good partial description of Desano (Tucanoan) morphology. (A more complete Desano grammar written by a missionary linguist with 35 years of experience in the region is now in press.)

[2]Early work such as Gomez (1980) has yet to come to the attention of theorists.

reaction to some of the failures of transformational-generative theory in the mid 1970's, which awoke current linguistic consciousness.

A crucial step in the awakening process transpired when 'in 1976, a professor in London was expounding on why no object-initial languages existed in the world. A student in the class hesitantly raised his hand and said, "Excuse me, but I *speak* an object-initial language." The professor was Geoffrey Pullum, and the student was SIL member Desmond Derbyshire' (Cahill 1999:1). This incident was the beginning of a series of publications (Derbyshire 1979, 1981,1985) which in interaction with the burgeoning field of typological studies has done more than any other previous development to bring the ignored and unknown languages of the Amazon languages to the lagging attention of theoretical linguists.

A simultaneous, even more significant step was the development of a fruitful editorial collaboration between the professor and his former student, beginning with the work Derbyshire and Pullum (1981) and eventually resulting in Derbyshire and Pullum (1986, 1990, 1991, 1998). At roughly the same time the two got underway producing these volumes, developments recounted in the introduction of Doris Payne (1990) transpired: thanks to some professors and graduate students at the University of Oregon who had worked in the Amazon region the complex classifier systems in some of the languages of this region were brought to the attention of theoreticians, first in Doris Payne (1986) (in the now classic Craig 1986), and then in two articles in Doris Payne (1990).

The next and still current surge of interest in Amazonian languages had its beginning with a renewed interest in the phenomenon of ergativity, most notably brought to the foreground of current theoretical consciousness in the papers of Comrie (1978), Dixon (1979) and the book by Plank (1979). Amazonian languages do not seem to enter into these earlier discussions; the year 1985 is the earliest date listed in Dixon (1994) for references discussing ergativity in Amazonian languages (in spite of earlier buried, provisional work such as Derbyshire 1983). Derbyshire and Pullum (1986), subsequent work by Derbyshire, and the involvement of the typological school at the University of Oregon (notably by the then Ph.D. student Spike Gildea) share major responsibility in bringing Amazon ergative systems to the attention of the wider linguistic world.

Some of these efforts came to the attention of the senior editor of the book under review, who was in part responsible for the simultaneous upsurge of interest in ergativity. He had, in his own words:

> ... devoted several decades to searching for substantive linguistic universals. In case after case, just as he thought he had achieved some significant typological

> statement, a counter-example popped up; and this was invariably from a language of Amazonia. He decided that the most sensible course of action was to learn Spanish and Portuguese and then go to South America … In this way he achieved a degree of insight into the most complex linguistic area in the world today. (3)

He also ran into the coeditor, who had in the late 1980's begun researching obscure Brazilian Amazonian languages. One happy result is the book under review; another is the emergence of a second professional team who, like Derbyshire and Pullum, enjoy similar prestige in linguistic circles, and who can hopefully help to push forward what the productive Derbyshire and Pullum team has begun. 'The Amazonian Languages' is Dixon and Aikhenvald's major step in that direction.

A casual reader who is not a specialist in the study of South American languages and who is not aware of these theoretical developments might be tempted to pass by this book. That would be a crying shame: in spite of the title, the book is relevant to linguistic investigators in all fields, because it is hard to stumble across an Amazonian language that does not have something to offer to theoretical issues currently in vogue. Not only does the book provide an overview of the major language families of the Amazon Basin, but it also describes minor language families and isolates. In addition, it gives a good basic (if somewhat brief) introduction to the area and study of Amazonian languages. Two of the chapters describe synchronic language contact situations. Almost all of the chapters provide historical background for the study of each family or group of isolates, and all of them reveal areas relevant to current issues in phonology, morphology, syntax, semantics, comparative and diachronic linguistics, and grammaticalization.

In this review article I briefly recapitulate the contents of each chapter, at the same time emphasizing characteristics unique to or unusual in the languages being described, and relevance to current issues in the field. After the content summary I comment on some more general issues which the book raises, and then evaluate the book as a whole.

1. Contents. The editors first discuss conventions used in the book, including spelling, naming of language families, definition of 'language,' grammatical terminology, following what Dixon calls 'basic linguistic theory,' i.e. 'the accumulated tradition of linguistic description that has evolved over the last 2,000 years' (xxvi). This is a welcome section, too often missing in linguistic books.

In the first chapter they provide a good basic introduction to the book, discussing topics such as the purpose of the book, the situation of scholarship in the region, 'cultural background' of the region, 'linguistic

diffusion' (areal linguistic features), proposed and likely genetic relationships, 'the punctuated equilibrium model' (see Dixon 1997), and the organization of the book.[3] The section on current linguistic scholarship and 'cultural background' are quite adequate for a linguistic anthology (that is what this book is), but anyone who desires or needs a more in-depth treatment of the cultural/historical situation of Amazonian languages should proceed to the excellent in-depth introductions in Derbyshire and Pullum (1986, 1990, 1991, 1998), where such information is more appropriate.

Although the author of each chapter has been given considerable freedom to describe language families according to the character of the respective linguistic systems, each chapter follows a similar format. Most begin with a brief summary of the history of studies in the family under consideration (including discussions of available classifications and comparative reconstructions), all provide maps of the location of language groups with accompanying population statistics, and all summarize the phonological traits typical across the family's languages. Each author then launches into a description of the language family's grammar. It is here that approaches differ significantly; nevertheless, morphology figures strongly in the majority of families, though it does not always appear under that title. Syntax receives attention where it is appropriate, and some authors even manage to squeeze in limited information on discourse grammar (text linguistics). Unfortunately, the authors had to observe rigid constraints on length for reasons outside of their and the editors' control; in spite of the incomplete descriptive coverage of Amazonian languages at present, even now a full book could be written on any of the seven families represented in the first eight chapters.

The first four chapters after the introduction cover the three largest language families in Amazonia, both in number of languages and of speakers: 'Carib' by Desmond Derbyshire (23–64); 'The Arawak language family' by Alexandra Aikhenvald (65–106); 'Tupí' by Aryon Rodrigues (107–124); and 'Tupí-Guaraní' by Cheryl Jensen (125–164). Curiously, Arawak, the largest language family, comes second in the book, but otherwise the order reflects the relative importance of these families both in the region and in current studies.

Derbyshire's chapter on the Carib family is what one would expect from a scholar of his calibre; his mastery of the details in this summary reflects over 40 years of exposure to Amazonian languages. In his review of available

[3] Readers who are familiar with the writings of both editors will detect the fingerprints of the senior editor all over this introduction.

classifications he notes that 'Carib comparative and historical studies lag far behind those of the other two large Amazonian language groups' (25). He does not dwell long on phonology: in Carib languages: the more interesting area is the interaction of phonological systems with the morphology, and it is here that phonologists will find plenty of fuel for theoretical fires, especially when considering the highly inflected verb systems.[4]

On the current linguistic scene Carib languages are most noted for their unique ergative systems. Because 'ergativity ... to a greater or lesser degree governs the case marking, person marking, derivational processes and constituent order patterning' (55), Derbyshire discusses it under several headings, including 'inflectional morphology' (31–37), 'main clause structure' (55), 'subordinate clause structures' (56–57), and a special section 'ergativity' (60–61). There are five 'dominantly ergative languages' (61), and a variety of split systems; the now well-known OVS (actually OVA) order occurs in ergative languages of this family. As in the Mayan language family (also known for ergativity), there is a close relationship between possessive nominal prefixes and verbal person affixes.

Derbyshire discusses at some length a current issue in the study of Cariban ergative systems: the two most distinguished scholars working in this language family differ on the origin and direction of diachronic change with respect to ergativity in the family. Derbyshire has argued elsewhere that earlier ergative systems are changing to accusative systems (Derbyshire 1991, also in the bibliography of this article), whereas Gildea (1998) argues that accusative systems are changing to ergative systems. Derbyshire provides a fair treatment of both sides, noting that 'Gildea's research has been extensive and his diachronic approach is sound and persuasive' (60), but at the same time he concludes that the issue cannot be satisfactorily resolved until 'fuller descriptions become available of more Carib languages,' which in turn will allow more extensive comparative studies and 'a more reliable internal classification of the Carib family' (61).[5]

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[4] For a sampling, see Gildea (1995), also in Derbyshire's bibliography.

[5] Derbyshire says that 'my view of the direction of change in the Carib family has been reinforced by a more general factor: the rampant ergativity that is found in so many Amazonian language families' (61). Ergativity is actually more wide-spread in Central and South American languages: I mentioned the Mayan family above, but languages in the Chibchan family (geographically intermediate between Mayan and Amazonian territory in pre-Conquest times) also offer a variety of ergative features in a generally decreasing cline to the south (see Quesada 1999 for a summary), and the Chocoan languages in western Colombia also sport ergative traits (Harms 1994). As one moves down the Andes, beginning in southern Colombia one runs into the thoroughly accusative Quechua languages. In evaluating Derbyshire's and Gildea's

Cariban languages offer other interesting characteristics for typologists, phonologists, and Chomskyan theoreticians: locative postpositions inflected for liquids, flat surfaces, open areas, and enclosed places (42–43); a brief mention of 'the particle word class,' which Derbyshire insists are not clitics but instead phonological words (53); and a rich array of nominalizing suffixes functioning syntactically as complements or adjuncts (43–52).

In a little over a decade Aikhenvald has managed to conduct extensive field work in Arawakan languages (mostly in south-western Brazil), and has an impressive command of the available literature. She is the only author who singles out endangered (vs. nonendangered) languages in her list of languages for the family (67–71), though comments on endangerment and survival prospectives for the respective families can be found sprinkled throughout the book. In her discussion of available classifications she notes that 'the first truly scientific reconstruction of proto-Arawak phonology ... was published by David L. Payne (1991). However, his subgrouping of Arawakan languages, which is based on lexical retentions, rather than on innovations, remains open to discussion' (74). David Payne has done work on possible shared morphological innovations across South American languages (David Payne 1990b, so that very likely he used retentions in an attempt to filter out the effects of pervasive areal diffusion. Aikhenvald's evaluation here, as well as her own work on areal diffusion in the Amazon (one sample can be found later in this book), raises the more general theoretical issue of how one can dependably classify languages and trace genetic relationships in areas where extensive diffusion has taken place.[6]

Aikhenvald's phonological summary (75–80) suggests that Arawakan languages have much to offer theoreticians in the area of phonology, including glottalization, aspiration and nasalization as word prosodies (79), and interactions of morphology with stress systems (for instance, in Asheninka 'monosyllabic verbal roots have an obligatory prefix or a suffix, to make them bimoraic' (80)). In fact, some of the stress systems have much exercised phonologists: Hayes (1995:288–296) presents a detailed reanalysis of the analysis in J. Payne (1990), concluding that 'this does not exhaust the stress phenomena of Asheninca' (Hayes 1995:296), and mentions the

positions, one should keep in mind that Cariban and Arawakan languages extended further to the north in pre-Conquest times.

[6] My unpublished classification of Tucanoan languages mentioned in Barnes' chapter in this book also uses shared retentions instead of innovations. I have put the manuscript aside, chiefly because the present pattern of spreading for innovations in the family is clearly due to language contact, which in turn suggests that I have reconstructed a former language contact situation, instead of a network of genetic relationships.

language repeatedly throughout his book. Asheninca stress figures in several papers in the Rutgers Optimality Archive, and in other current literature; it has become a testing ground for current phonological theory. Other languages in this family appear to offer theoretical challenges in this area: for instance some, such as Achagua, assign stress according to grammatical word class.[7]

Arawakan languages also offer much of interest to typologists: inalienable and alienable possession (with 'cross-referencing prefixes' (82–83)); unique, complex, multiple classifier systems in interaction with gender systems (some languages have three interacting classifier systems, and one, Palikur, has five) (83–84); and complex verb morphology, including cross-referencing prefixes and suffixes (two-thirds of the languages), split ergative systems (typical of most Arawakan languages with cross-referencing suffixes), abundant valency-changing derivations, complex tense-aspect, modality, directional, and aktionsart systems (82–94).

Rodrigues is a fitting author for the more general description of the Tupí language family; he has over 40 years of experience working in the Amazon, and is currently the leading Tupían comparativist. Excluding the Tupí-Guaraní branch Tupían languages have been poorly described, or not described at all; Rodrigues makes do with what is at hand and provides an overall introduction to the entire family. Outstanding characteristics of these languages include inalienable and alienable possession, strict transitivity-based verb classes, 'rich systems of demonstratives' (120), 'subordinate clauses ... achieved through nominalization' (121), and some ergative characteristics (chiefly 'pivots' (121)).[8]

In her chapter Jensen, who has over 20 years of experience in the Amazón, and who is also a Tupían comparativist, presents a summary for Tupí-Guaraní, the best known and best described branch of the Tupían family. Her historical-geographical introduction repays careful study; of all the families described in this book, European colonists learned to speak and used a few of the more prevalent languages exclusively from this family in preference to their native tongues (see 125–133). She devotes a considerable amount of space to Tupí-Guaraní phonology (133–145), perhaps best known among phonologists for a variety of unique nasal prosody systems, some of them bidirectional (135). Other traits of interest to phonologists include vowel

[7] I base this statement on Achagua data that I have seen and analyzed. A phonology is in rough draft, coauthored by a Colombian linguist and an expatriot colleague.

[8] See Dixon 1994 for discussion of 'pivots' with respect to ergative systems.

epenthesis or consonant loss across morpheme boundries (the latter interacts with metathesis) (136), and voicing of bilabial consonants at morpheme boundries (137). Word-final consonants can devoice, disappear, or become nasals (142–144).

Traits of interest to typologists include split ergative systems interacting with the person hierarchy (O has precedence over A). The system includes four complex set of person markers interacting with transitivity and local discourse topic while marking A (subject of an active transitive verb), O (object of a transitive verb), S_a (the subject of active intransitive verbs), and S_o (the subject of stative intransitive verbs), 'the genitive in nouns,' and/or 'the object of postpositions,' (146–148).

The next three chapters cover four less extensive, smaller language families which nevertheless are significant in Amazonian linguistics: 'Macro-Jê' by Aryon D. Rodrigues (165–206); 'Tucano' by Janet Barnes (207–226), and 'Pano' by Eugene Loos (227–249). Rodrigues emphasizes comparative studies in his chapter on Macro-Jê (165–166, 198–201), because the internal consistency and external relationships of this group have been much questioned by area specialists. Nevertheless, there are also goodies which will interest the non-specialist, such as nasal prosody systems (in most of the languages oral and nasal vowels contrast, 171–174). One language (Karajá) has men's and women's speech (176–178). Plural marking is parsimonious (non-existent in the Jê family), sometimes appearing on pronouns but usually not on nouns; in some languages plural (A or O) is marked within the verb stem (183–184). Some ergativity occurs; Rodrigues' summary suggests that more work is needed to determine its exact nature (193–195).

Barnes' chapter on Tucanoan languages reflects roughly 30 years of experience in the Vaupés of Colombia, and immediate access to dozens of linguists who have similar experience studying these languages. She discusses Tucanoan classification using an unpublished manuscript of mine, basically an update of an older classification (Waltz and Wheeler 1972, in her bibliography). All Tucanoan classifications and reconstructions are clouded by hundreds of years of extensive internal diffusion between Eastern Tucanoan languages, raising the theoretical question of how one can classify languages within a family, and reconstruct a proto-language in such a situation. The sociolinguistic situation in the Vaupés is unique among all contact situations in the world; more details can be found in references in Barnes' bibliography and in Aikhenvald's chapter in this book on diffusion and language contact in the Içana-Vaupés area (see below).

Tucanoan languages have much to offer the typologist and phonologist: they sport the most complicated, highly inflected evidential systems in the world (at least among the world's languages that have been studied); most have

rich classifier systems (suffixes on numbers, demonstratives, nominalized verbs, and some nouns) in interaction with a gender system; almost all have nasal prosody; and across the family a variegated array of poorly described pitch-accent or accent systems begs for comparative study. Nasal prosody in Tucanoan is difficult to describe without postulating 'three-way autosegments;'[9] in spite of some interesting proposals within Optimality Theory, it is questionable whether this theoretical quandary has been resolved. Curiously, the Tucanoan languages are all thoroughly nominative-accusative in a region where ergativity is dominant.

Loos' chapter reflects over 40 years of experience with Panoan languages. Panoan phonology is complex (230–234); it is perhaps most distinguished by 'nasal spread' and complex interactions of morphology with the metrical system, i.e. 'an odd-even syllable-timing characteristic common in Pano languages causes phonological modifications such as segment deletion, plosive nasal release, stress assignment and possibly vowel harmony' (232).

For typologists there are 'a variety of split [subject-marking] systems, the marking of the A and S being affected by focus' (236) within a system of 'transitivity concord' (some subordinate clauses, some adverbial verb suffixes, locative phrases locating A or S, and certain sequential clauses must be marked according to verb transitivity). There is a complex switch reference system, perhaps the most complex in the Amazon, and best exemplified by Sparing-Chávez 1998 (described for Amahuaca, not available to Loos at the time of writing). Ergative marking occurs, uniquely marked by a syllable final nasal /n/ which often disappears and leaves nasalization as its only clue; in turn, 'in some of the languages the nasalization has been lost' (240). Some languages have (apart from all the above) an impressive variety of verb suffixes; 'in some languages more than 130 verb suffixes are available' (244). The chapter ends by briefly describing a complex system of deictics.

Three chapters cover diminutive families: 'Makú' by Silvana and Valteir Martins (251–268); 'Nambiquara' by Ivan Lowe (269–292); and 'Arawá' by R.M.W. Dixon (293–306). Though the Makú family is now small, 'four languages belonging to seven tribes' (251), Makú speakers were the original inhabitants of the Brazilian Upper Rio Negro (and Colombian Vaupés) regions, later conquered and displaced by Arawakan and then Tucanoan groups. The phonology is quite distinct from surrounding languages, offering

[9] Barnes lists a paper in her bibliography describing how these segments operate (Barnes 1996), both for pitch and nasal prosody in Tuyuca, but she does not mention this fact in the chapter, perhaps due to limited space.

an abundance of CVC words (in a region where most languages are strongly CV oriented) and more complex vowel systems, except for Kakua, which seems to be heavily influenced by the surrounding Tucanoan languages.[10] Phonologies are actually more complex and fascinating than this summary would suggest: Yuhup has nasal harmony, phonetic pre- and post-nasalized voiced stops (Brandão Lopes and Parker 1999).[11] Daw, Kakua, and Yuhup have tone, in all cases incompletely analyzed.

Makú languages have much to offer the typologist: inalienable and alienable possession; locative postpositions which some might interpret as classifiers because 'their choice depends on the physical properties of the referent of the head' (258); noun incorporation (only in Nadeb); and ergativity (Nadeb, and possibly Kakua—the other languages are nominative-accusative). Within the Makú family, Nadeb stands out glaringly: this matter is discussed somewhat in Aikhenvald's chapter on areal diffusion in the Içana-Vaupés basin.

The tiny Nambiquara family (in Brazil) also has nasal prosody (not mentioned as such by the author), three contrastive tones, and contrastive laryngealization on nasal and oral vowels. Resyllabification of underlying consonant-final verb and noun roots occurs when these are suffixed. In the area of typology these languages offer well-developed evidential systems, exceeded in complexity only by those found in Eastern Tucanoan languages. There is also a complex system of verb subordination suffixes, noun classifiers (occurring on nouns, as 'deverbal nominalizers,' modifying adjectives in an NP, and numerals), and a set of subject, object and copula pronoun suffixes (including dual number). Lowe includes more on clause and interclausal syntax than do most authors in this book (277–279, 284–289); this has long been a special emphasis of his research.

The most notable characteristic of wider interest in the small Arawá family is probably the complex split ergative system, in which discourse topic, person, and noun suffixes all interact. The resulting construction then determines (for the most part) constituent order (299–300, 304–306). The languages have gender, and feminine is unmarked (masculine is considered to be marked). Some nouns 'require a cross-referencing prefix /ka-/ on the verb and on some nominal modifiers (when the noun is in pivot function in the clause)' (298). This is intriguing, in light of the /ka-/ of Arawak languages which has diffused into some Tucanoan languages; according to the analysis

[10] There is an factual error in the text here (256, second line). Tucanoan languages have six vowel systems; Kakua does have a five vowel system, as the authors state.

[11] This reference was not available to the authors.

in Metzger 1999 one of the major functions of /ka-/ in Tucanoan and Arawakan is to indicate that a specific individual, item, or group is being focussed on. Some Arawakan languages are spoken in the general region where Arawá languages are found, though it does not look at first glance like much diffusion has taken place.

Two chapters discuss small language families and language isolates: 'Small language families and isolates in Peru' by Mary Ruth Wise (307–340); and 'Other small families and isolates' by Alexandra Y. Aikhenvald and R.M.W. Dixon (341–384). Both chapters represent a synthesis of the most disparate data to be treated in the book. Wise's discussion of Peruvian languages shows an amazing command of detail, in addition to skill at organizing what looks at first glance like hopeless disarray; it reflects long experience in the region (roughly 35 years) and previous practice in synthesizing large amounts of data (see Wise 1990 for just one nice example). With respect to phonology Wise observes that 'most of the languages in the five families differ from areal patterns in one or more traits' (312), all carefully listed (312–318). The most interesting to phonologists likely will be nasal prosody (here she refers to the published version of David Payne 1974), tonal systems (tone is not that common in the Amazon, as this book attests),[12] pitch accent systems in which stress (intensity) and high pitch (accent) do not necessarily coincide (described for Aguaruna in David Payne 1990a, but according to Wise, probably typical of all Jivaro languages), and a nasal /h/ which nasalizes vowels that follow it (in Arabela).[13]

Most of the languages are rich in morphology. Characteristics of interest to typologists include classifiers, mostly numerical (one Witotan language, Bora, has over 350, and uses them with pronouns, in addition to the more usual nouns, demonstratives, adjectives and verbs). Dual number, unusual for the Amazon (as Wise notes) appear in the Witotan languages and Yagua; in Murui Witoto interaction with a three-way gender system results in a complex pronominal system. Some evidence suggests ergative traits in Jivaroan languages, but this possibility has not been fully explored. Verb

[12] A fine resource now available for phonologists interested in tone systems of these languages (not available to Wise at the time of writing) is Walton et al. (1997); tone is marked on all entries in this dictionary. The third coauthor is a native speaker of Muinane, a member of the Witoto family.

[13] Walker and Pullum (1999) discuss this segment in a short report in Language, in which they argue that phonetically possible (or pronounceable) segments should not be excluded from phonology on theoretical grounds (see 769).

morphology is especially complex, but the categories expressed are more or less typical for the Western Amazon.

Aikhenvald and Dixon's skills in dealing with complexity and disparity rival those of Wise; the wide distribution (Brazil, Bolivia, Colombia, and Venezuela) of small families and languages they summarize, plus a scarcity of sources, has made their job even more difficult. This variegated array of poorly described or basically undescribed languages certainly underscores the point made in the editors' introduction concerning the lack of attention given to Amazonian languages by the current linguistic world.

Languages covered which should be of interest to non-Amazonian linguists include the Yanomami dialect continuum (distinguished by 'a rich system of verbal classifiers' (347–348), multiple verbal proclitics and suffixes (over 20 on each end), extensive noun incorporation ('any noun in S or O function' (350)), and extremely productive verb compounding. Mura-Pirahã phonology is only briefly mentioned, but it is certainly one of the more unusual ones to be found in the book, with an unusually reduced consonant inventory (354) and a complex, as of yet only partially analyzed tonal system (according to Everett 1986).[14] The most well-known feature (among theoretical phonologists), that of syllable weight partially determined by consonant onsets, is not even mentioned here (see Everett and Everett 1984a, b).[15] Pirahã is also somewhat unusual that it lacks formal marking for tense, plurals, and possession.

The authors discuss the Guahiboan languages (Colombia and Venezuela) and some language isolates found in Colombia and Venezuela. Guahiboan languages are unusual in a number of areas: 'suppletive forms of verbs' depending on the number of the A or O (372); complex classifier systems (used with numerals, adjectives, and deictics) (373); 'an unusually large number of oblique cases compared to other Amazonian languages' (375); 'some traces of split ergativity, of an active-stative type;' incorporation of inalienably possessed nouns, either in S function with verbs of physical state, or S, O, or oblique function with other verbs.[16]

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[14] To my knowledge, nothing has appeared since which would indicate otherwise.

[15] These references are not in the bibliography of this chapter. Readers who want to know more about Mura-Pirahã phonology and who do not read Portuguese should look at 308–325 of Everett (1986) (which is in the bibliography, and which does mention the role of consonant onsets in determining syllable weight).

[16] A couple of important references on the Guahibo language (Kondo 1985 and Queixalós 1985) do not seem to have been available to the authors, probably due to inaccessibility.

The last two chapters discuss two linguistic areas characterized by linguistic and cultural diffusion across several language families: 'Areal diffusion and language contact in the Içana-Vaupés basin, north-west Amazonia' by Alexandra Y. Aikhenvald (385–416); and 'The Upper Xingu as an incipient linguistic area' by Luci Seki (417–430). Both chapters will be of interest to sociolinguists and linguists specializing in language contact. Aikhenvald's chapter contains an impressive amount of linguistic detail, much of it based on her own fieldwork in both Arawakan and Tucanoan languages. The discussion covers most areas of phonology, morphology, and syntax; the author even includes a brief comparison of 'syntax and discourse techniques' (405–406). In contrast, Seki offers very little linguistic data; her intent apparently is to alert potential researchers to the possibility of rigorously documenting diffusional history from its beginning in a region where such work could be a real boon to struggling comparativists.

Diffusion in the Içana-Vaupés basin is most distinguished by the almost exceptional unidirectionality: the diffusion is from Eastern Tucanoan languages to geographically contiguous Arawakan and Makú languages. The only currently known exception is the Arawakan prefix /ka-/ 'relative, attributive' (Aikhenvald mentions this prefix in a footnote on p. 392 and in her chapter on Arawakan languages—see p. 95).[17] The unidirectionality is most certainly a reflection of sociolinguistic factors, as the author notes.

2.0 Comments on more general issues. Several issues raised by the editors' comments in the section on conventions and the book's introduction need more comment for the benefit of those who have not worked extensively in the Amazonian basin. I comment on these issues in the order in which they appear in the book, and I include an issue not covered which should be brought to the attention of those who wish to pursue field work in the Amazon or use scholarship from the Amazonian area (perhaps as a result of reading this book).

2.1 Terminology for language families. Specialists in South America when writing in English put an *–an* ending on the names of most language families. In Spanish, they do not, but instead write 'the family X,' where X is the name without any adjectival (or other ending). In this book the editors follow the Spanish convention when writing in English. In many cases this means that a language family and a language in the same family will have

[17] A detailed description of the occurrence and function of /ka-/ in Tucanoan and Arawakan languages is found in Metzger (1998) (not available to Aikhenvald at the time of writing).

the same name. They insist that context is enough to distinguish between the two, but my own experience suggests that this is not always the case.

In fact, I had never given the whole business much thought, until opening this book. When operating in Spanish, I (like most colleagues) automatically use the Spanish convention, and when using English, I follow the English convention. One must not only be bilingual with respect to vocabulary and grammar, but also with respect to conventions of language use. In this review article I have followed standard English convention, except when I quote from the book. It is noteworthy that in previous work and in at least one unpublished manuscript I have seen postdating the book the junior editor uses the standard English convention.[18]

2.2 Communication between missionary and Latin American linguists. Although the editors' observations are amazingly accurate regarding this topic, they are incomplete. After long years in Latin America, it appears to me that neither side has told the editors the whole story, perhaps because both sides know the editors communicate with anybody and everybody they can find working in the field, no matter what their religion, politics, or nationality (plus the junior editor, aside from being a polyglot, has an outstanding gift for relating to most anybody). For one thing, the Judeo-Christian ethics of most missionary linguists would preclude recounting to the editors negative incidents in a long history of ups and downs as they have sincerely tried to relate to local scholars and academic structures—all this in the face of concerted regional negative publicity, which extended from the seventies until the early nineties decade, and still (as the editors note) lingers in the region.

On the other hand, Latin American scholars are reluctant to comment on the highly politicized environment of too many universities, which has sometimes resulted in negative attitudes to the work of non-Latin linguists being a necessary part of maintaining one's job and position. In Latin America it is much harder to find jobs and funding that allow one to describe these languages; that does not help matters. Fortunately, the funding situation is beginning to change, due in part to the founding of several

REVIEWS

Extended finite state models of language. Ed. by ANDRÁS KORNAI. Cambridge: Cambridge University Press, 1999. Pp. 284. Cloth $59.95.

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This volume contains eighteen papers which were originally from a workshop held in August 1996 in Budapest as part of the European Conference on Artificial Intelligence. Finite state (henceforth FS) models are well-defined and understood mathematically and have been used in many morphological parsing systems, including PC-KIMMO (Antworth 1995). This volume contains papers that describe extensions to the standard finite state notions. While this is not a book for the typical field linguist, it does hold interest for computational linguists, especially those who use and appreciate finite state methods. It even comes with a CD ROM containing source code and tools corresponding to many of the articles.

The volume begins with the editor's overview of the volume, 'Extended finite state models of language.' Joshi and Hopely's 'A parser from antiquity: an early application of finite state transducers to natural language parsing' is a description of a parsing system developed in 1958–9 that used a number of features that are currently considered to be modern. Kartunnen's 'Comments on Joshi and Hopely' sheds further light on the implications of this paper for current work.

The volume then turns to some practical development issues. Watson's 'Implementing and using finite state automata toolkits' describes a toolkit written in C++. Vilares, Graña, and Alvariño's 'Finite state morphology and formal verification' deals with verifying the properties of a large morphological analyzer.

Tateno, Masuichi, Umemoto's 'The Japanese lexical transducer based on stem-suffix style forms' describes their work on a full-size lexical transducer for Japanese. Next is Kim and Jang's 'Acquiring rules for reducing morphological ambiguity from POS tagged corpus in Korean.' It focuses on efficiency issues.

The volume then turns to syntactic parsing using finite state technologies. Chanod and Tapanainen's 'Finite state based reductionist parsing for French' describes a system that deals with multiword expressions. Grefenstette's 'Light parsing as finite state filtering' describes a system that marks simple noun and verb phrases. Kornai's 'Vectorized finite state automata' has an FS

extension that he uses to extract relational information for English. Roche's 'Finite state transducers: parsing free and frozen sentences' applies FS means to find idioms and nominal subcategorizations.

FS methods are applied to machine translation in Vilar, Jiménez, Amengual, Castellanos, Llorens, and Vidal's 'Text and speech translation by means of subsequential transducers.' Ejerhed's 'Finite state segmentation of discourse into clauses' applies FS techniques to finding discourse clauses in text. Schulz and Mikołajewski's 'Between finite state and Prolog: Constraint-based automata for efficient recognition of phrases' describes a technique for employing FS methods with constraint-based ideas and applies it to syntactic agreement in German texts. Bangalore's 'Explanation-based learning and finite state transducers: Applications to parsing lexicalized tree adjoining grammars' augments a lexicalized tree adjoining grammar with FS methods. Tzoukermann and Radev's 'Use of weighted finite state transducers in part of speech tagging' explains the utility of weighted FS transducers.

The volume then turns to several further extensions of the mathematical formulism. Csuhaj-Varjú's 'Colonies: A multi-agent approach to language generation' describes how an FS extension called colonies can provide context sensitive power. Nederhof and Bertsch's 'An innovative finite state concept for recognition and parsing of context free languages' introduces meta-deterministic languages which they claim deal with inherently ambiguous languages. Finally, Ristad's 'Hidden Markov models with finite state supervision' applies FS techniques to a machine training paradigm.

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A reference grammar of Dutch; with exercises and key. By CAROL FEHRINGER. 1999. New York: Cambridge University Press. Pp. xxiii, 185. Hardcover $69.95, paper $27.95.

Reviewed by TERRY MALONE
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Why bother to review a reference grammar of Dutch in a journal tailored mostly to the needs of linguists studying unwritten or non-IndoEuropean languages?

First of all, it is a reference grammar, and no better arguments encouraging linguists to dally with reference grammars can be found than in Dixon 1996.

Secondly, in spite of the title, the author has written it for language learners. She states that 'the purpose of this book is to provide an accessible reference grammar of the Dutch language for English-speaking students of Dutch and to help consolidate their knowledge through practical exercises on a whole range of grammatical topics' (vii). It can serve as a model or a source of ideas for linguists engaged in writing pedagogical works. In fact, it would seem more appropriate to title it 'A reference grammar of Dutch for learners,' or 'A pedagogical reference grammar of Dutch.'

But either alternative might put off a third possible group of users: linguists who need a succinct but thorough overview of basic Dutch grammar in order to evaluate theoretical arguments based on Dutch data.

The arrangement of this grammar is highly unusual: grammatical topics are presented in alphabetical order. Although in twenty-five years of practicing linguistics I had never heard of nor seen a grammar organized this way, in a footnote (vii) the author mentions another reference grammar (for English) organized alphabetically.

Readers no doubt can imagine the problems that alphabetical organization could engender for users of a reference grammar, especially students who know nothing about grammatical terminology or linguistics. Even linguists might stumble: one is tempted to barely glance at the introductory material, head straight for a category such as 'pronouns' in the text, and augh!—find no entry under that word.

For readers who do not skip the introductory material the author has neatly circumvented such problems, by providing an alphabetical list 'Grammatical terms' at the beginning of the book. In the list under 'pronouns' one finds a simple definition (aimed at non-linguist types) and in parentheses the advice 'see demonstrative pronoun, indefinite pronoun, personal pronoun, possessive pronoun, relative pronoun.' Other categories are treated similarly, and as a result attentive users should be able to quickly find what they are looking for.

The book is generally learner-friendly. The author recommends looking up grammatical points of interest in the index, which lists entry and subentry numbers, then proceeding to the table of contents. This includes not only entry numbers and the page number where the entry is found, but also the page number of exercises designed to reinforce the grammatical points illustrated by the entry. The carefully constructed exercises (119–158) are graded from beginning to advanced competence, from recognition and comprehension to full production. A full key is provided (159–176)—even

for the most advanced exercises, which mostly involve composition, sentence construction, answering questions, and dialogue construction.

The author, editors and publisher are to be commended for the editorial care lavished on this book.[1] Cross-referencing is extensive—even in the exercises, which in themselves are of better than average pedagogical quality. Footnotes appear on the bottom of the page (instead of buried somewhere in the back of the book). The modern features of word-editing programs have been used in a way which notably enhances user-friendliness: titles and numbers of subentries are in white letters against dark gray; separate fonts are used for grammatical stipulations, Dutch examples, explanations in English and English translations of Dutch examples; grammatical constructions focussed on in Dutch examples are in bold; tables and charts are easy to read and well-organized, with black text on a light-gray background; and titles of the current entry appear at the top of each page. The book is worth buying just as an example of how to format and organize a reference grammar.

The publisher's blurb states that this book can also be used by those who for whatever reason wish to teach themselves Dutch in a non-academic situation. Obviously, one does not learn the grammatical categories of a language in alphabetical order. The author therefore provides a 'didactic guide for beginners ... suggesting which topics they should tackle first in order to attain at least a basic level of Dutch' (xvii–xix).

This particular approach to language learning favors certain learning styles, chiefly those of analytical, visually oriented learners,[2] or in the terms of Stewick 1989, 'deliberate' (possibly also 'self-aware') learners. It is almost certain that this type of learner can learn to at least read Dutch using this book.[3] The multiple examples provided for almost every entry and subentry are a big help here; the examples reflect actual speech patterns and are always accompanied by English translations in the main text.

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[1] I only found one typo in the English text, on p. 1 (I can't authoritatively speak for Dutch, but I saw no obvious errors).

[2] For some explanation of these terms see Carol Orwig's 'Language learning strategies' or 'Ways to approach language learning' in LinguaLinks 4.0: Electronic helps for language field work (Dallas, TX: SIL International, 1999).

[3] I fit in this category. I found that by the time I had finished the book, I was able to understand bibliographical entries in Dutch and Dutch examples in linguistic works much better than beforehand. Nevertheless, I am not an ideal guinea pig for using this grammar as a pedagogical resource, because I enjoy daily exposure to two different varieties of spoken German. According to Koenig 1978 by the 1700's Dutch had assumed enough of a separate identity that it could be considered to be an independent language separate from neighboring low German dialects (103).

Readers who have been keeping an eye on theoretical literature know that Dutch speaking linguists and Dutch grammar have played significant roles in theoretical developments in the last twenty five years or so. How does this book stack up for linguists who want to use it as a resource while plowing through complicated theoretical arguments sprinkled with Dutch data?

Several areas of Dutch grammar have been brought to bear on theoretical arguments. Two that come quickly to mind are impersonal passives, and the auxiliary choice between *zijn* [sein] 'to be' and *hebben* [hɛbʌ] 'to have.' The work of Perlmutter (1978, 1984) in a relational grammar framework using Dutch examples (along with Italian and German, and to a lesser degree, other languages) offered a syntactic analysis of the Dutch impersonal and passive constructions, forced practitioners operating in Chomskyan frameworks to face weaknesses of their theory in these areas, and was chiefly responsible for the wide acceptance of the terms 'unergative' and 'unaccusative' in several current linguistic theories.

Fehringer's grammar deals with these areas, but one must know where to look; the appropriate sections are '21 *Er*' (25–27), '33 Irregular Verbs' (42–47) (in which verbs which exclusively take 'to be' and verbs that allow either 'to be' or 'to have' in the passive are carefully marked), '46 Passive' (61–63), and '48 Perfect tense' (65–69). The debate between syntactic and semantic analyses of the Dutch passive auxiliaries is ongoing: Lieber and Baayen 1997 provides a useful summary of the current debate and a nice technical supplement to F's description and examples in her sections 'Irregular verbs' and 'Passive.'

Another major area of Dutch grammar which has been brought to bear on current theory is that of word order.[4] F devotes a whole section of her grammar to word order (entry 78, pp. 111–116); it is one of the longest entries in the grammar. The entry has cross-references to other sections which bear on word order: 'Objects—direct and indirect,' 'Reflexive verbs,' '*Er*,' and '*Niet*.' A sampling of the literature suggests that interested linguists should also look at '17 Conjunctions' (19–20), '30 Infinitives and use of (*om*)...*te*' (37–38), '32 Inseparable verbs' (40–41), '40 Modal verbs' (50–52), and '64 Separable verbs' (99–100). The word order entry ends with a brief discussion of 'deviations from the usual word order,' some of which have figured significantly in recent theoretical arguments (see Odijk 1998 for an example). Articles such as Corver 1997 may require forays into more advanced grammars (F suggests several in a footnote on p. viii) or even consultation with native speakers, but in most cases F's grammar should

[4] References are too numerous to include here.

provide the basic framework a linguist needs to evaluate word order discussions involving Dutch examples.

The book has no major faults and few minor faults. The latter mostly turned up when I showed the book to a guest in my home who happens to speak Dutch as her mother tongue. We went through the pronunciation chart together (entry 58, p. 89) and found a few slight differences in some of the vowels.[5] We searched through the introductory material for a statement of the variety of Dutch represented in the book, but found only this statement at the beginning of entry 58: 'this chapter provides a brief summary of standard Dutch pronunciation. It is intended as a rough guide only and does not take regional and other types of variation into account.' But nowhere does it mention that standard Dutch is the variety spoken in Amsterdam. Apparently the author assumes that the student should already know this.

Another small omission turned up when my guest looked at the first grammatical entry, 'Accents' on p. 3. The text talks about using accents to distinguish the indefinite article *een* 'a' from the number *een* 'one' (*een* and *één*, respectively). But it does not mention that the pronunciations are also different, roughly [ʌn] (mid open central vowel) and [e:n], respectively.

Nevertheless, these are small quibbles for a fine book, and a useful one, even for linguists hopelessly in love with non-IndoEuropean languages.

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[5] The chart in the book gives the pronunciations of standard Dutch sounds in IPA symbols. Thanks to Sonja Merritt for helping check out this chart and sharing a bit of her mother tongue (a variety spoken in Amsterdam).

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Lexical and constructional aspects of linguistic explanation: Studies in Constraint-Based Lexicalism. By GERT WEBELHUTH, JEAN-PIERRE KOENIG and ANDREAS KATHOL. Stanford, CA: CSLI Publications. 1999. Pp. xi, 401. $64.95 hardback, $24.95 paperback.

Reviewed by MIKE MAXWELL
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This book presents a convergence of two theories of syntax: Head Driven Phrase Structure Grammar (HPSG), and Constructional Grammar. Historically, HPSG is a successor to Generalized Phrase Structure Grammar, and as such represents a reaction against the use of transformations in generative grammar. Constructional Grammar also represents a reaction against traditional generative grammar, in that constructions—passives and auxiliary verb inversion, for example—are treated not as more or less accidental outcomes of other principles (movement or conditions on binding, among others), but as fundamental objects in the theory.

The twenty two papers (not including the introduction) of this volume are revised versions of talks given at several workshops held between 1994 and 1997. As one might expect, given the workshop audience, the papers are not for those unfamiliar with HPSG (although very little knowledge of Constructional Grammar is presupposed), nor for the unconverted. (However, a few papers, including Wechsler's (see below), do address the issue of HPSG vs. other generative theories.)

It is beyond the scope of this interview to discuss all the papers, but a brief overview of some of the topics should give a flavor: ergativity, incorporation, quantifier floating, topicalization, antecedent contained ellipsis,[1] short- and long-distance reflexives, the adjunct vs. complement distinction, topicalization and *wh*-movement in German-type languages,

[1] For those unfamiliar with this term, a classical example of antecedent contained ellipsis is the sentence 'John read the (same) books that Mary did.' The relative clause 'that Mary did' is presumably contained inside the VP 'read the books that Mary did,' while at the same time 'did' seems to be followed by an empty (elided) VP. If the semantics requires that this elided VP be filled in in order to be interpreted, a naïve approach would copy the antecedent VP into the position of the elided VP. But since the antecedent VP contains the elided VP, there would seem to be an infinite regress.

agreement, strong generative capacity, and computational implementation. A typologically wide variety of languages is covered, ranging from American Sign Language to West Greenlandic (and of course, English). While this breadth of issues is a strength—everyone will find something of interest—it is also a weakness: the overall collection is likely to be too broad for most readers. Meurers and Minnen's article on computational implementation, for example, assumes a knowledge of the Prolog programming language, and a familiarity with the details of the HPSG analysis argument raising in German. The audience for such a paper is (unfortunately) quite small.

Most of the papers assume the reader understands the abbreviatory conventions and notation of HPSG (such as the difference between '<>' and '<[]>'). While a few authors do provide definitions for the notational conventions, the index is not much help for finding those definitions. There is also inconsistency in usage (in part due to the fact that the papers cover several years' worth of developments), e.g. Wechsler's article uses the term 'a-binding' throughout, while Manning and Sag use the term 'o-binding' for exactly the same concept. (The change in terminology is, however, mentioned in the Manning and Sag's footnote 1.)

Despite the book's title, perhaps less than half the papers actually treat 'constructional aspects of linguistic explanation.' One exception is the paper by Charles Fillmore, 'Inversion and constructional inheritance,' which explicitly takes a constructional grammar approach—but without trying to show how such an approach could be melded with HPSG. Fillmore's paper, which takes as its topic the various constructions in English which exhibit subject-auxiliary inversion, should nevertheless be clear (and thought provoking) to linguists of any background.

The editors' introduction gives a very brief overview of the theory and the papers, but those who have not kept up with the changes which HPSG has undergone during the last decade will want to read Manning and Sag's admirably clear 'Dissociations between argument structure and grammatical relations' before tackling the other articles. Manning and Sag explain the reasoning behind dividing up the work done by the subcategorization list in earlier versions of this theory, into an 'argument structure list' on the one hand, and a set of three valence features—Subject, Complements (a list), and Specifier—on the other. This division is assumed (and in part, argued for) by many of the other papers in this volume. Manning and Sag illustrate how anaphor binding, passivization (at least in some languages), and ergativity are handled under this approach.

In the space remaining, I will mention but two more of the papers in this collection, papers which I found to be both clear and enlightening.

Stephen Wechsler's article 'HPSG, GB, and the Balinese bind' treats binding of anaphors in Balinese. Like many Austronesian languages, Balinese verbs can take as their subjects any of a number of arguments, not just the highest in a thematically ranked list (which is often the agent). The resulting clauses are distinct from passives (which Balinese also has). However, while verbs can take almost any argument as subject, regardless of its thematic ranking, the antecedent of an anaphoric pronoun is always the thematically highest ranking argument, regardless of whether this argument is realized as the subject. Wechsler shows how these facts can be analyzed in both HPSG and GB (Government Binding) models. Up to this point, one might see the two analyses as notational variants, a claim which is sometimes made. But then Wechsler brings in additional data from raising in Balinese, and extends the HPSG analysis to account for the new data. He further shows that it is difficult to imagine how a GB analysis could handle this data; the HPSG analysis cannot be imported into GB, and the other GB analyses Wechsler offers fail. This argument might not be convincing—proponents of one theory often overlook ways another theory might account for the data—but Wechsler describes work by a GB proponent, Lisa Travis, who attempts to account for similar data in a related language. Her analysis requires radical changes to GB, changes which strike at the heart of what GB is.[2] Unfortunately, I suspect that generative linguists may not pay this article the attention it deserves, since GB has been superceded by Minimalism—unfortunate, since Wechsler's main point (that what initially appears to be a notational variant, may not be) does not depend on the particular theory.

Andreas Kathol's paper 'The scope-marking construction in German' looks at the most common way long distance WH-questions are formed in German. Unlike long distance WH-questions found in English (and marginally in German), German prefers a construction with two WH-phrases (Kathol's (2a)):

what believe you with who Jakob talked has
Was glaubst du [mit wem Jakob geredet hat]?
'Who do you think Jakob talked with?'

Most previous analyses of this construction have assumed a link between the upper clause *was* 'what' and the lower clause WH-phrase—for example, that *was* is an expletive, and that at some semantic level (Logical Form, for example), the expletive is replaced by the WH-phrase. Kathol's construction based analysis is that the embedded WH-phrase terminates a slash

[2] For example, it becomes necessary to define binding on theta-positions rather than on A-positions.

dependency[3] (thereby filling the missing argument of the embedded clause), but also starts a new slash dependency, to be filled by the higher *was*. (A paraphrase of the above question would thus be 'What do you think? Who did Jakob talk with?') Kathol shows how his construction-based HPSG formalization (based on an earlier analysis by another linguist) explains a number of additional syntactic facts, such as the incompatibility of this construction with negation.

This volume will probably not be on most linguists' 'must buy' list, since it will be the rare individual who will wish to read more than a few of the papers.[4] It would however be a good addition to a library.

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Lexical relations. By JEAN-PIERRE KOENIG. Stanford Monographs in Linguistics. Stanford, CA: CSLI Publications. 1999. Pp. 213. Hardback $49.95, paperback $18.95.

Reviewed by MIKE MAXWELL
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A common assumption in linguistics, dating back to Bloomfield, is that the lexicon is simply a listing of irregularities. Koenig stands this assumption on its head, and argues that many of the properties of language derive from the structure of the lexicon. That by itself would not be a new idea—many linguists now reject Bloomfield's conjecture—but K makes his proposal more specific by proposing that the structure of the lexicon is based on an inheritance network containing underspecified lexical entries, with the properties of words being determined by 'category intersection.' What exactly these terms mean, and whether such a lexicon can account for not only the regularities (productive generalizations) and irregularities of word structure, but also for everything in between (i.e. semi-productive lexical relationships) is the topic of this book.

To begin with, in K's view (and that of many other linguists), the lexicon is structured as a multiple inheritance network, with categories and words populating the nodes of the network. In an inheritance network, the

[3] 'Slash dependencies' are the GPSG and HPSG of propagating the information about a 'gap' from the location of the gap to its WH-filler, and are partly analogous to intermediate traces of WH-movement in many transformational generative accounts.

[4] A quick search revealed that several of the papers in this volume (but not all) are available on the Web. One wonders how much longer it will be economically viable to produce printed volumes of this sort.

properties of any given node depend in part on the nodes from which it 'inherits' information, and in part on information provided by the particular node.

A network with SINGLE inheritance is tree-structured. That is, any given node inherits properties directly from a single immediate super-node (and indirectly from any super-nodes of that node). In linguistics, one might postulate a single inheritance hierarchy for parts of speech. The part of speech 'transitive verb' would then be a sub-type of the part of speech 'verb,' inheriting properties which all verbs share (the ability to be marked for tense, for example), while at the same time specifying other properties which are limited to transitive verbs (such as the ability to take a direct object).

Where there are cross-cutting generalizations, it becomes necessary to go beyond SINGLE inheritance to MULTIPLE inheritance. In languages with inflectional (paradigm) classes, for example, it is often the case that each inflection class will contain both transitive and intransitive verbs. Any given verb (more accurately, any verbal lexeme—i.e. the verb in the abstract, without inflectional affixes) must therefore inherit properties from two nodes: from the class of transitive or intransitive verbs, and from one of the inflection classes.

When one looks at language more closely, it becomes apparent that lexemes inherit a number of cross-cutting properties. Hence it is generally agreed among linguists who accept the need for a structured lexicon that multiple inheritance, not single inheritance, is necessary.[1]

Under K's theory, the daughter nodes of a particular node can be in one of two kinds of relationships: an AND relationship, or an (exclusive) OR relationship.[2] If a set of daughter nodes is in an AND relation, then a lexeme chooses a value from each daughter node. For example, if verbs are marked for both tense and aspect, the tense node and the aspect node would be in an AND relationship with each other. If on the other hand, a set of daughter nodes is in an OR relationship, this indicates that a word selects only one such daughter node. An example of this would be the daughters of a tense

[1] Feature systems are equivalent to multiple inheritance systems (a similar point is made in Hudson 1971:72). To take a simple example from phonology, the fact that some sound is [+voiced –strident] is equivalent to saying that the sound inherits the properties of voiced sounds and the properties of strident sounds. Feature co-occurrence constraints, such as that between [+nasal] and [+strident], are then statements about illicit multiple inheritance.

[2] It took me several readings to understand how K's notation distinguishes between AND nodes and OR nodes. A row of boxed nodes represents an AND choice, i.e. it is necessary to make a choice for each such boxed node. A row of un-boxed nodes represents an OR choice, that is, it is necessary to choose ONE of the un-boxed nodes.

node, which might be past, present, and future; only one such daughter would be chosen for a particular inflected verb.

Up to here, there is nothing new about K's approach; indeed, his lexical inheritance network with AND/OR nodes bears a striking resemblance to the networks developed in the 'systemic' school of linguistics (see e.g. Hudson 1971, ch. two[3]). Where K's theory departs from other theories, is in its use of underspecification. The notion of underspecification is familiar from generative phonology, but its use in the grammar may be more novel. The motivation for underspecification in the lexicon is to allow productive lexical relations to be left implicit, rather than explicitly marking them, or deriving them by lexical rules. In the absence of lexical rules, K's theory allows new categories to be formed on the fly by 'conjoining,' or intersecting, existing compatible categories.

Roughly, two categories are compatible if none of their values conflict. As an example, most count nouns in English can be either singular or plural. Suppose the meanings of singular and plural are expressed as inheritance from a singular node or a plural node, both of which are features of type 'number.' These nodes are in an (exclusive) OR relation. That is, it is possible to inherit from one or the other, but not from both. Most noun lexemes (stems) would be stored with this number property uninstantiated. In order to form the singular or plural of such a regular lexeme, the noun lexeme would be conjoined with (inherit from) either the singular or the plural node. However, some nouns—*plurialia tantum* words like *pants*, *eyeglasses* and *scissors*—exist only as plural forms. The lexemes for these nouns would be stored with the inheritance line from the plural node already instantiated (specified), thereby blocking inheritance from the singular node. This would be an extensional solution, since each noun in this set would be individually linked to the plural node.

But K's theory allows another solution. For at least some *plurialia tantum* words in English, there is a language-specific generalization: nouns referring to 'lower trunk wear,' such as *pants*, *shorts* and *trousers*, are always plural. K attributes the informal generalization to Williams, but this set can be encoded (more)[4] formally in K's system as a sub-type of count nouns, the members of which are determined intensionally, in terms of their meaning.[5] That is, there is no need to specify for each individual item in this set the fact

[3] However, systemic linguists did not restrict their networks to lexical properties.

[4] In the absence of a theory of semantics that provides a way to refer to 'lower trunk wear,' the statement must remain semi-formalized.

[5] The fact that the class is determined intensionally makes it possible to explain the fact that when the nouns *Levis* and *cords* (in the sense of 'corduroy pants') entered the English lexicon, they did so as plurals.

that it belongs to the *plurialia tantum* class. Instead, the number of these nouns can be left uninstantiated in their lexical entries; the information will be filled in automatically by inheritance from the semantic properties of 'lower trunk wear.'

At the same time, there are other nouns which are *plurialia tantum*, but whose membership in this class must be extensionally marked, such as words like *eyeglasses*. This is compatible with K's theory; the fact that a given property is marked extensionally for some words does not prevent it from being marked intensionally for other words. Moreover, the theory provides an interesting path for the language learner from extensional generalizations to intentional ones. For instance, when the child first learns that the word *pants* has no distinct singular form, she must store this fact extensionally; there can be no generalization for a single word. Later, she may learn that *shorts* and *slacks* also lack distinct singular forms, and mark them extensionally in her lexicon. At this point, she may—or may not—discover the intensional generalization, and represent that generalization as a subclass of count nouns in her lexicon. The ability of the theory to encode such generalizations in either extensional or intensional form, and to provide a learning path between them, is a factor in its favor.

Another interesting aspect of K's theory is his analysis of an affixed word as INHERITING certain properties from the affix, without committing to an analysis of the word into stem and affixal constituents. K contrasts his approach, which he calls a 'construction-based approach,' with the traditional 'morpheme-based approach.' The traditional approach treats *player* as being made of the morphemes *play* and *-er*, whereas K's approach analyzes *player* as inheriting properties from the lexeme *play* and from the properties common to the set of agentive *-er* nouns. K's construction-based approach allows him to analyze the adjective *aggressive* as an *-ive* adjective despite the absence (in most dialects) of the root **aggress*; *aggressive* is simply a lexeme which inherits the properties common to *-ive* adjectives. (However, explaining how the relation between *aggression* and *aggressive* is to be represented is more difficult, as K admits.)

K claims as a further advantage for his construction-based approach the fact that if 'phonological information is allowed on non-terminals, there is no need for terminals to record this information' (92). That is, the word *player* need not be analyzed into the terminal string *play* and the terminal string *er*. But the distinction between terminal and non-terminal information is, so far as I can tell, merely terminological (pun intended): phonological information is no more terminal in a morpheme-based approach than in K's construction-based approach. Putting this differently, whether something appears to be terminal depends on what dimension you concentrate on. Indeed, both the

categorial (morphosyntactic) dimension and the phonological dimension are, under closer inspection, multi-dimensional objects, with terminals composed of morphosyntactic and phonological features, respectively.[6] What does differ between the construction-based approach and the morpheme-based approach is not whether the phonological strings are terminal, but whether the phonological representation of the word is exhaustively analyzed into disjoint phonological representations, one for each constituent morpheme.

A shortcoming of this book is that K often resorts to analyses of a partial set of data, and it is not always clear that his approach would scale up to more data. For example, on pp. 78–80 he discusses passives in English, and in particular exceptions to passivization. Pre-theoretically, such exceptions fall into two classes: a positive exception is a verb which appears only in the passive, such as *rumored*, while a negative exception is a verb found only in the active, e.g. *have*.[7] K's explanation is that most verbs are underspecified for the VALENCE property, and can therefore assume either the TRANSITIVE or the PASSIVE value of this property; positive exceptions are lexically specified as having the PASSIVE value, which blocks their assuming the TRANSITIVE value, while negative exceptions are lexically pre-specified in the opposite fashion.[8] Thus, since *rumored* is prespecified for the PASSIVE value of the VALENCE property, it is incompatible with the TRANSITIVE value of that same property.

But in fact, PASSIVE is not a valence: the category PASSIVE is not in opposition to the category TRANSITIVE. Rather, passivization is a relation between two different valences, such that one valence has one fewer NP arguments than the other valence. For example, in English there are not only passives of transitives, but also passives of verbs which in their active form take an NP and a PP argument (*A book was given to Mary*), passives of verbs which take an NP object plus a sentential complement (*Spot was told that he was a bad dog*), passives of raising and control verbs (*The story was believed to be true*, *John was persuaded to leave*), passives of ditransitive verbs (*Scrooge was paid a visit by three spirits*), etc. The passivization of ditransitive verbs is particularly problematical for K's treatment of passive as

[6] Dimensions in phonology are commonly referred to as 'tiers.'

[7] While the verb *have* is commonly cited as a negative exception, it can in fact appear in the passive in certain senses, for example *A good time was had by all* and *You've been had*. The significance of these exceptions to the exception is unclear; possibly different senses are simply linked to different sets of morphosyntactic properties.

[8] Koenig discusses another approach to the active-passive distinction, which relies on a feature (Koenig calls it an 'attribute') VOICE, with two possible values of active and passive. Positive exceptions would be lexically specified as [VOICE passive], while negative exceptions would be pre-specified as [VOICE negative]. It is unclear to me whether this is more than a notational difference. In any case, Koenig does not make much of the difference between the two approaches, real or not.

a kind of valence: with respect to argument-taking properties, the passive of a ditransitive verb *is* transitive,[9] and thus must be allowed to inherit from both of these supposedly mutually exclusive values of the VALENCE property.

Given that passive and transitive are cross-cutting properties, is it possible to analyze the English passive in terms of K's AND-OR graphs? In his discussion of the French verb *vendre* 'to sell' (70ff.), K suggests that the category of passives in French[10] is one in which the list of syntactic arguments is one item shorter than the list of semantic arguments. Under the general approach on which K bases his theory, argument lists are ordered lists, with the least oblique element (the subject) appearing first in the list. Hence the omission of the stem's first argument from the argument list results in what was the direct object of the active stem becoming the subject of the passive, leaving the remaining arguments undisturbed. This analysis of the French passive avoids the problems with K's analysis of English passives; indeed, the solution is quite similar to one lexical rule solution,[11] with the exception that in K's analysis, PASSIVE is a static category (not a rule), which can be further unified with other categories. I suspect something like this analysis would work for English.

Another shortcoming of this book is that K's comparisons between his theory and competing theories are not always straightforward. For example, in an analysis of Latin verbal inflection, K claims that to form a fully inflected Latin verb, one conjoins (ANDs) the properties of a root with the properties chosen from (ORed from) each of three inflectional categories. In the diagram (K's figure 51, p. 81), these categories and choices are: aspect (perfect or non-perfect), tense-mood (past subjunctive or past indicative), and person (first singular actor or second singular actor). K contrasts this with Anderson's (1992) analysis, which—K claims—substitutes conjunctive rule blocks for K's AND choices, and disjunctive rule blocks for the OR choices. However, I believe this is a mischaracterization of Anderson's theory. Under K's analysis, what is being chosen is the values of

[9] In some dialects of English, there are two passives of ditransitives: *John was given a book* and *A book was given John*. While one might argue that one or the other of these is not transitive (with the NP following the verb being something other than an 'ordinary' direct object), it seems that at least one must be transitive.

[10] Actually, K's analysis is of the French 'medio-passive' (see his figure 47, p. 73). But the English passive is similar enough that the same approach could probably be brought to bear.

[11] A similar lexical rule analysis is given in Briscoe and Copestake (1999). The principle difference between a lexical rule analysis and K's analysis, is that the lexical rule analysis relates two different lexical entries, whereas K's analysis relates the passive form to the active stem contained *inside* the passive form's lexical entry. Briscoe and Copestake's analysis of the passive also differs in that the demoted subject is explicitly attached at the end of the passive's argument list (as a *by*-phrase).

morphosyntactic features. But Anderson's approach assumes the feature values are given, and uses conjunctive and disjunctive rules to choose inflectional affixes. Features (or sets of features) and affixes are not, at least according to Anderson, in a one-to-one relation: one affix may realize more than one feature, and a given feature may be realized by more than one affix. To the extent that there is not a one-to-one relation between features and affixes, Anderson's theory and K's are not directly comparable.[12]

The volume seems to be quite free of typographical errors,[13] and reasonably well indexed (although a few terms coined by K are not indexed).

In summary, this is a provocative work. In my opinion, however, the theory needs to be worked out in more detail for valence-changing relationships. In particular, the treatment of passives needs to be developed for a wider range of English verbs, and extended to other sorts of valence-changing operations in other languages.

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Second language learning theories. By ROSAMOND MITCHELL and FLORENCE MYLES. London: Arnold. 1998. Pp. 228.

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Overview of content. In 1987 Barry McLaughlin wrote *Theories of Second Language Learning*, an influential summary and critique of theories of Second Language Acquisition (SLA) current at that time. A decade later Rosamond Mitchell and Florence Myles have written a new book whose stated aim is to provide an up-to-date introductory overview of SLA research and to provide a successor to McLaughlin's 1987 book. M&M follow

[12] K might object that in his theory, it is not single features that are chosen, but bundles of features—'first singular actor,' for example. But so far as I can tell, the opposite situation (multiple exponence, where a single feature is represented in more than one affix) is not readily represented under this interpretation.

[13] There is a minor mixup in glossing in example 25, (44), easily corrected.

McLaughlin's example in dividing the book into sections dealing with theories with linguistic, psycholinguistic and sociolinguistic perspectives, although M&M have added some areas of research that have appeared over the past ten years, particularly those viewing the language learning process as essentially social.

The opening ch. provides an overview of key concepts and issues to allow the reader to compare the goals and claims of the different theories. Ch. 2 is a review of SLA research since the 1950s. The following chs. address the various approaches to SLA:

- The Universal Grammar approach
- Cognitive approaches
- Functional/pragmatic approaches
- Input and interaction in second language learning
- Sociocultural perspectives
- Sociolinguistic perspectives

In ordering the chs. in this way the authors move from perspectives based on linguistics to those grounded in the social sciences, and from a focus on the nature of language and the mental representation of language in the mind of the individual learner to a focus on language as a means of interaction and communication within society. Each ch. ends with an evaluation of the theories according to the claims and scope of the theory, the nature and extent of the empirical research and the views of language, the language learning process and the learner involved in the theory. A final ch. summarizes the advances in SLA research and looks ahead at future directions for SLA research. I comment on a few selected chapters in what follows:

Theories based on Universal Grammar. Ch. 3 focuses on theories of SLA based on linguistic theory, and in particular on Universal Grammar. These theorists see the core of the language learning process as the setting or resetting of parameters within a 'hard-wired, genetically based Universal Grammar (UG), of which every human being has their personal copy.' The language data in the surrounding social environment provides evidence that guides the setting of UG parameters.

After briefly summarizing the concepts of principles and parameters in UG, the authors discuss four theoretical positions about SLA in relation to UG taken by SLA theorists. The first position is that UG is available only to first language learners and is no longer available to adult learners of a second language. The second position is the opposite: that UG is fully accessible to L2 learners as it is to L1 learners. Proponents of a third view, the INDIRECT

ACCESS position, claim that learners have access to UG only through L1; in other words, they have already accessed the principles and have set the parameters to L1, which then serves as the basis for learning L2. Finally, the position that has most recently come to the fore is that of PARTIAL ACCESS, in which some aspects of UG may be available to L2 learners and some not, although there is lack of agreement as to which are which.

In any case, in their evaluation of UG-based approaches to SLA theory the authors point out that UG is a theory of language, not a theory of learning, and that it must be judged for what it is. Still, UG has had a big impact on research in the field and will probably continue to do so.

Cognitive theories. Whereas the theories based on UG concentrate on WHAT the learner is learning, the ch. on cognitive approaches to SLA presents theories based on HOW the learning takes place. Many of these approaches are based on cognitive psychology and tend to view SLA as one kind of learning within the broader context of human learning and believe that general learning mechanisms can account for much or all of the processes of language learning. One interesting but controversial theory in this section is CONNECTIONISM, or parallel distributed processing (PDP), which likens the brain to a computer consisting of neural networks. Connectionists claim that language is not rule-governed, but is based on the formation of associative patterns. Their research is mostly controlled laboratory research, involving experiments with artificial languages or fragments of real languages. It attempts to build links with neurology and to study learning from within the architecture of the brain. The controversy comes from the fact that researchers do not assume underlying representations of language and a special Language Acquisition Device in the mind of the learner.

M&M view connectionism as an exciting and promising new avenue for research, but point out that it is questionable how much we can learn from lab experiments about language learning in naturalistic situations.

Another influential group of cognitive approaches to SLA is the set of information-processing models. One of the first of these is McLaughlin's information processing model, which views second language learning as the acquisition of a complex cognitive skill, in which simpler component sub-skills become automatic through practice. Over time, as performance improves there is restructuring of the internal representations in which learners simplify, unify and gain increasing control. Unlike Stephen Krashen and his followers, McLaughlin and other proponents of cognitive approaches see no clear distinction between language acquisition and language learning. Indeed they see no clear distinction between language learning and other types of human learning.

Another processing model from cognitive psychology that has been applied to second language learning is Anderson's ACT model (adaptive control of thought). Like McLaughlin's model it deals with how practice can lead to automatization and how declarative knowledge (knowledge about something) can lead to procedural knowledge (knowing how to do something). In their critique of cognitive models M&M point out that they are helpful in understanding how classroom knowledge can turn into language proficiency and particularly, how fluency develops but are less helpful in showing what the mental grammar of learners consists of.

Functional/pragmatic approaches. Functional/pragmatic perspectives on SLA view language development as driven by pragmatic communication needs and the urge to communicate meanings in social settings. Over time more formal aspects of language are developed to express more complex meanings. Some of these studies are based in part on T. Givón's distinction between pragmatic and syntactic models of expression. Some early studies, such as one by Dittmar argued that the conversational talk of some elementary adult learners of a second language showed more characteristics of the pragmatic mode, rather than the syntactic mode, especially as regards theme-rheme. Later studies attempted to show how the syntactic mode might develop and serve as a test of Givón's theory.

Social approaches to SLA. Although differing from each other in significant ways all the approaches mentioned thus far have concentrated on how language develops within the language learner as an autonomous individual. The theories surveyed in the remainder of the book view language learning much more as a social activity in which the social environment interacts with learners' innate learning capacities to produce language acquisition.

Research on input and interaction focus on the NEGOTIATION OF MEANING between language learner and fluent interlocuter as a key to the language acquisition process. Other theorists go even further and claim that target language interaction is not just a source of 'input' for individual innate learning mechanisms, but itself constitutes the learning process. Sociocultural theory is based on the work of Vygotsky's sociocultural theories of child development. The book ends with a consideration of some of the diverse sociolinguistic perspectives on SLA, including the ethnography of L2 communication, variation in second language use, pidginization and acculturation.

Concluding remarks. In this book the authors have provided an accessible and intelligible introduction to a variety of approaches to research in SLA and insightful commentary. The book would have been even more useful if

each ch. had ended with suggestions of key articles or research in the field, though there are references within the text to bibliography entries. Still the authors have succeeded in their aim to provide a successor to McLaughlin's 1987 book, and it is recommended for anyone wanting a clear and broad introduction to the field of SLA research.

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Language death. By DAVID CRYSTAL. Cambridge: Cambridge University Press. 2000. Pp. 198. Hardback $19.95.

Reviewed by PETE UNSETH
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Crystal has produced a rare book on at least two counts. First, it can be read profitably and enjoyably by both linguists and the wider public. Second, it is a linguistic book written with passion, as C vigorously raises the alarm about the high and growing number of languages that are in danger of extinction and calls for aggressive action to reverse this trend. Just as activists around the world are calling for protection and support for endangered biological species, C calls for action to support endangered languages.

Language death happens when nobody speaks a language any more, usually due to language shift, as people from the cultural community do not pass on their traditional language to the next generation. Sometimes community members see no reason to preserve their language, but activists see this as an attitude that outsiders should work to change. Clearly, there is a clash of viewpoints in many cases.

The organization of the book is straightforward, progressing from information to motivation to specific actions. The five chs. are: 'What is language death?' 'Why should we care?' 'Why do languages die?' 'Where do we begin?' and 'What can be done?' C's writing style is easy to follow, leading the reader logically from point to point.

In a book with the title *Language Death*, one expects a multitude of statistics on endangered languages, factors that may lead to language death, possible steps to save and strengthen languages. C provides all these, of course. But what surprised me was his list of reasons to value individual languages, with a much greater emphasis on affective rather than utilitarian values. This was

laced with a very generous selection of fascinating quotations on the value of individual languages and mother tongues, from sources as varied as Ralph Waldo Emerson, Martin Heidegger, and Marianne Mithun.

In working to revitalize a language, C stresses the need for a team approach, with members of the speech community as foundational, with linguists also having important roles, including advising on orthography, dictionary preparation, selection of dialect, describing the grammar, and helping people to value their languages. But linguists must let the language community make policy decisions, and be prepared to make concessions on such things as restricting the distribution of language materials.

What are some factors that can contribute to stabilizing and even strengthening the vitality of a language? C's list includes literacy in the language, raising the prestige of the language and the ethnic group (both within the community and in the wider society), raising the economic level of the language community, having a strong presence in the national educational system, and making use of electronic technology. He might have added improving the physical health of a community (see Cahill's report on the SIL website, mentioned below). None of these factors is a guarantee that a language will revive or continue, but all of these factors contribute in varying degrees to a language's vitality.

It is surprising that C barely refers to SIL, which has had a long and supportive involvement among hundreds of communities of endangered languages. He does, however, make reference to the Ethnologue repeatedly, as well as cite the writings of some SIL linguists. Some of SIL's impact and current thinking about the topic can be found at SIL's website:

www.sil.org/silewp/1999/006/SILEWP1999-006.htm
www.sil.org/SOCIOLX/NDG-LG-GRPS.HTML#DoWhat

with links to other sites on the topic. As in his earlier book, *Linguistic Controversies*, C is still publishing the fallacy that SIL is dominated by Tagmemics (153), which has not been true for decades. In a book that is otherwise characterized by careful documentation and a balanced tone, it was also disappointing that he made harshly negative comments about some missionaries without giving any documentation (84).

This book will be of obvious interest to field linguists, and would be useful for any linguistics or anthropology library. It also deserves to be read by the broader public, where there is growing interest in the process and consequences of cultural encroachment and language death.

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Adverbs and functional heads: A Cross-Linguistic Perspective. By GUGLIELMO CINQUE. New York and Oxford: Oxford University Press, 1999. Pp. xii, 275. Cloth $45.00.

Reviewed by JOOST ZWARTS,
BTL—Kenya

This book is one from a series *Oxford Studies in Comparative Syntax*, edited by Richard Kayne, which has a very strong minimalist (formerly 'principles and parameters' and 'government-binding') orientation. This book exemplifies the bold view on language variation, characteristic of much of the minimalist work done in the wake of Kayne 1994 and Chomsky 1995, that all languages have the same underlying phrase structure and differ only in how and where the elements of the structure are spelled out. More specifically, all languages have the same inventory of functional categories (including such exotic categories as Celerative for affixes that express that the action was done quickly) and these categories project a universal X-bar structure, with at most one specifier preceding the head and one complement following it: [$_{XP}$ Specifier [$_{XP}$ X Complement]]

The claim of the book is that an adverb occupies the specifier position of a particular functional category that covers the same semantic domain. For example, the adverb *already* is the specifier of a functional Tense (anterior) category and *allegedly* is the specifier of a special evidential Mood projection. Cinque proposes that a sentence has about thirty different Mood, Modality, Tense, and Aspect categories and that languages can either express these categories through their heads X (as affixes or particles) or through their specifiers (as adverbs). His proposal is based on the observation that adverbs and affixes seem to show the same hierarchy across languages.

For those who favor a more economical syntactic representation, this sounds like an 'outrageous' idea, to use C's own term. Not many linguists will agree with C that all possible modal, temporal, and aspectual distinctions have to be hard-wired in the syntactic knowledge with which children are born. However, apart from this aspect of the book the proposal shows some interesting similarities with ideas that have been expressed elsewhere about the universal ordering of affixes and adverbs across languages (Foley and

Van Valin 1984, Bybee 1985, Hengeveld 1989), but it seems more likely that semantic and functional principles underlie this ordering.

In Ch. 1 'On the relative order of adverb phrases,' C uses data from French and Italian and a few other languages to argue that adverbs come in the same fixed order in every language. For example, frequency adverbs like *often* will universally be closer to the verb than modal adverbs like *perhaps*. This ordering might be obscured by factors involving scope, focus, or movement. Ch. 2 'A case for adverb phrases in SPEC' presents the hypothesis in a few pages using verb movement patterns in Italian. Ch. 3 'On the order of clausal functional heads,' shows how the ordering of functional heads like Mood, T(ense), and Asp(ect) can be motivated by data about the ordering of affixes and particles from a wide variety of languages. The information about adverbs and functional categories is combined in Ch. 4 'Matching and refining the hierarchies of adverb phrases and functional heads.' Functional categories that do not have a fixed position (various agreement categories and negation) are dealt with in Ch. 5 'DP-related functional projections and negative phrases'). Ch. 6 'Some implications and residual questions,' is devoted to the feature analysis of the more than thirty clausal functional categories, their semantics, and possible extensions of the hypothesis to the structure of noun phrases, prepositional phrases, and adjective phrases.

There are two appendices, one of which is an overview of the orders of (overt) functional heads in a wide variety of languages, ordered into families. This might be a useful resource for those that are working on morphological order phenomena, but some care should be observed in taking all the data at face value, because, as field workers know all too well, it can be very difficult to give a definite classification of a morpheme as 'past tense' or 'perfect aspect' or to determine the kind of modality involved in a particle. After an extensive section of 'Notes and references,' the book provides a 'Language index, Name index, and Subject index.'

The main usefulness of the book is that it brings together data about the hierarchies of adverbs and verbal affixes in one model. Even though most of us would not formalize the common hierarchy in the same syntactic terms, we can be quite sure that such a hierarchy exists and realizing that mood, modal, tense, and aspect morphemes do not occur in a random fashion will benefit our work as (field) linguists in making sense of the patterns of morphemes that we encounter.

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Negation and polarity: Syntactic and semantic perspectives. Ed by LAURENCE R. HORN and YASUHIKO KATO. New York and Oxford: Oxford University Press, 2000. Pp. x, 271. Cloth $85.00, Paper $29.95

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Negation is a fascinating subject. From a logical point of view, negation is the simplest possible operator, only changing the truth value of the sentence it works on. If *It is raining* is true, then *It is not raining* is false, and vice versa. If natural language was not more complicated than the calculus of propositions in this respect, there would have been no reason for this book. However, negation has many syntactic, semantic, and pragmatic aspects that keep intriguing many linguists, as witness this book.

Contrary to what logic leads us to expect, two negative elements in a sentence do not always cancel each other, but can instead reinforce each other, as in *I can't get no* satisfaction. This so-called NEGATIVE CONCORD might be substandard in English, but it is a common phenomenon in some languages. Ljiljana Progovac in her contribution 'Coordination, c-command, and "logophoric" n-words,' argues that the relation between the two negative elements is akin to the binding relation between a reflexive pronoun and its anaphor. She uses a special notion of logophoricity to account for why reflexives and n-words can sometimes remain without a c-commanding antecedent.

Another surprising phenomenon is that of negative elements that do not seem to contribute a negation to the meaning of the sentence, as in the exclamative German sentence *Was du nicht alles machst!* 'The things you do!' Paul Portner and Raffaella Zanuttini in 'The force of negation in WH exclamatives and interrogatives,' argue that this so-called expletive negative DOES contribute negative meaning, but that the negative force is obscured by pragmatic implicatures.

NEGATIVE POLARITY is one of the central issues in the book, mentioned in almost all the contributions. It involves items like *ever* or *to lift a finger* that

can only occur in certain kinds of sentences, the most prominent among which are negative sentences. In 'Negative polarity items: Triggering, scope, and c-command,' Jack Hoeksema gives a useful overview of negative polarity and a critical evaluation of the role of c-command in licensing negative polarity items (NPIs). 'Pick a theory (not just ANY theory): Indiscriminatives and the free-choice indefinite' by Laurence R. Horn is an entertaining article about the interpretation of one prominent NPI (*any*), especially in constructions with *not just/not only*.

While negation in logic only takes scope over sentences, negative elements in natural language can also apply to other constituents. A sentence with a negation can be ambiguous because of these two scope construals (e.g. *John would be happy with no job*). The relevance of this ambiguity for syntactic structure (in minimalist terms) is the topic of the contributions of Liliane Haegeman, 'Negative preposing, negative inversion and the split CP,' and Yasuhiko Kato, 'Interpretive asymmetries of negation.'

Even without negative elements a sentence can still have a 'negative flavor,' because the use of spatial terms can be metaphorically extended. In his paper 'Negative inference, space construal, and grammaticalization' Masa-aki Yamanashi argues that the near-equivalence of sentences like *That exceeds my authority* and *That is not my authority* in English and Japanese can be explained by grounding negation in the cognition of spatial notions of boundedness, containment, and orientation.

Ladusaw's article is in some sense the odd one out. It was not among the original presentations and most of it is really not about negation but about thetic (stage-level) versus categorical (individual-level) predications. A temporary state description like *being available* is of the first kind; an example of the second kind of predication is *being intelligent*, which is a description of a more permanent property. The relevance of this distinction for the analysis of negative concord is discussed briefly.

The book has a section 'Further reading' with an up-to-date overview of more than nine pages of literature about negation. There is an 'Index of names,' an 'Index of topics,' and an 'Index of languages.'

For field linguists beginning to investigate negation in the language they study, this is probably a useful book to start with. It will give them a good overview of the important current issues.

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REPORT

New Theoretical Perspectives on Syntax and Semantics in Cognitive Science

Dubrovnik, Croatia, September 2–10 2000

Conference report by JOHN R. ROBERTS
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Preamble. I have not had the opportunity to attend a linguistic conference for a number of years. I was therefore especially pleased to find that this conference was stimulating, refreshing and very much the start of something new and interesting for me in the world of structural-functional syntax and semantics.

The conference was actually a five day course followed by a three day conference. The main purpose of both the course and the conference was to promote Role and Reference Grammar (RRG) as expounded in Van Valin and LaPolla 1997. The University of Zagreb also had a secondary purpose to promote the International Center of Croatian Universities at Dubrovnik as a venue for international conferences in Linguistics and Cognitive Science. At the opening reception we heard that the Minister of Science for Croatia no less had raised funding for the conference. The Mayor of Dubrovnik was also there to welcome all those who had come to participate in the conference. In fact, the friendliness and helpfulness of the Croatian organizers was one of the endearing qualities of this conference. The organisers could not have chosen a more beautiful setting for the conference, on the Adriatic coast right next to the medieval walled city of Dubrovnik.

Overview. There were 60 participants including those who presented courses and those who presented papers in the conference. Most of the participants were from Croatia, but others came from Brazil, Canada, Denmark, Germany, Holland, Italy, Japan, Macedonia, Poland, Russia, Spain, U.K., and U.S.A. The course section had five presentations lasting over five days. Each presenter was given three 90 minute sessions for their course spread over three days for each. These presentations were:

Melissa Bowerman (Max Plank Institute, Nijmegen): 'Crosslinguistic perspectives on first language acquisition.'

Daniel L. Everett (SIL International, Brazil): 'Word-formation in linguistic theory: A case for violable constraints.'

Ranko Matasović (University of Zagreb): 'Synchronic and Diachronic Typology of Syntactic Structures.'

James Pustejovsky (Brandeis University) 'The Generative Lexicon and Semantic Theory.'

Robert D. Van Valin, Jr. (SUNY at Buffalo): 'Syntactic Structures.'

During the conference section twenty-eight papers were presented in half hour sessions over the final three days. Most of these papers related to RRG in some way, but a wide range of linguistic topics were addressed including child language acquisition, sign language, prototypes and language typologies, lexical semantics and synonymy, the nature of universal grammar, clitics and adpositions, grammatical function changing devices such as dative shift and antipassive, focus constructions, complex sentences and clause linkage, metaphors and proverbs, and diachronic syntax. Typically, the papers focussed on particular languages. These included Croatian, Danish, English, German, Hebrew, Italian, Japanese, Macedonian, Madi (Brazil), and Spanish, as well as five minority languages spoken in the Russian Federation, viz. Altai-Kizhi, Chukotkan, Ket, Khanty, and Tuvan. The linguistic presentations of Altai-Kizhi and Tuvan were made by native speakers of those languages.

The courses. I found the courses I followed best were those for which I had done some reading and research beforehand. This included 'Syntactic Structures' by Van Valin and 'The Generative Lexicon and Semantic Theory' by Pustejovsky. In the course 'Synchronic and Diachronic Typology of Syntactic Structures' Ranko Matasović assumed that his audience was familiar with theories of diachronic syntax in IndoEuropean. Since I was not I found this presentation the most difficult to follow. I give below a brief review of each course.

'Syntactic structures' presented by Robert Van Valin. Robert Van Valin began the courses with 'Syntactic Structures.' He presented the basic concepts in RRG using case marking as a phenomenon found in language that requires a theoretical explanation. Specifically, the theoretical issues addressed were:

> What determines the case of an NP: constituent structure, grammatical relation, or something else?
>
> How is irregular case handled, such as found in split-S and fluid-S languages or more exotic case-spreading and case-stacking?
>
> How is long-distance case assignment handled, as found in Icelandic, for example?
>
> How is case assignment in complex sentences handled, especially in (monoclausal) causative constructions and in control constructions?

The particular aspects of RRG that Van Valin touched on were the layered structure of the clause based on the language universal oppositions of

predicate + arguments vs. non-arguments, the distinction between constituent projection and operator projection, lexical representations as decompositional representations based on the *Aktionsart* distinctions in Vendler 1967, the semantic macroroles of 'actor' and 'undergoer' as first proposed in Foley and Van Valin 1984, and focus structure based on the information theory in Lambrecht 1994. In his last session Van Valin presented some interesting cases of case spreading (the same case marked on more than one nominal in the clause) and case stacking (two different cases marked on the same nominal) in Korean, taken from Park 1995 and Han 1999 and showed how this could all be accounted for in terms of focus domains.

The phenomena of case spreading and case stacking are fairly rare cross-linguistically, but surprisingly Natalya Koshkaryova showed with the presentation of her paper that both occur in the Siberian language of Khanty. This was also accounted for in terms of focus domains.

'Word-formation in linguistic theory: A case for violable constraints' presented by Daniel L. Everett. Daniel Everett presented three papers discussing various aspects of morphology theory. The first paper, based on data from the Amazonian language Wari, challenged the Lexical Integrity Hypothesis (LIH) as maintained in Chomsky 1970, 1982 for example. Wari has constructions which express thoughts and intentions. The verb stem is represented by the quotation clause, i.e. the 'thought,' which is followed by inflectional verb morphology. The whole construction carries word stress instead of clause stress and thus functions as a word. Similar constructions occur in many Papuan languages, such as Amele for example (see Roberts 1987). In effect you have a clause embedded inside a word which contravenes the LIH. Everett argues that any current formal theory of morphology has difficulty in accounting for the Wari forms. Roberts 1996 presents a whole range of Amele forms where clauses are embedded within words and derived forms are dependent on inflectional forms. The Wari forms are readily accounted for in RRG which has a predicate position in which anything functioning as the predicate can be mapped.

Everett's final session was a presentation of Everett 2000. Here Everett proposes dealing with the clitic vs. affix distinction in terms of treating pronominal clitics as morphological adjuncts and pronominal affixes as morphological complements. The morphological structure is:

$[X^{\circ} [X^{\circ}] Agr^{\circ}]]$ (Agr° = 'clitic')

$[X^{\circ} Agr^{\circ}]]$ (Agr° = 'affix')

This conception of 'clitics' and 'affixes' as grammatical functions rather than lexical primitives has numerous advantages over other perspectives. For

one thing, it immediately accounts for, in fact predicts, the well-known clitic vs. affix diagnostics proposed in Zwicky and Pullum 1983. In RRG pronominal verb agreement and pronominal cliticisation in pro-drop languages are treated as arguments of the verb instead of the free nominals in the clause.

'Crosslinguistic perspectives on first language acquisition' presented by Melissa Bowerman. Melissa Bowerman is a world authority on child language acquisition and has substantiated the semantic basis of syntactic structures as proposed in RRG. In her first presentation she looked at child language acquisition as a form-function mapping problem: Where do the meanings children link to the forms and patterns of their language come from? Are the concepts known beforehand? In looking at the conceptual domain of space and how children match this concept to the forms in their language the evidence is that children know about spatial concepts long before they acquire the words to express those concepts. However, children associate verbs and phrasal verbs with language-specific categories of space before two years of age and they get the hang of the typological pattern of their language as early as two. So there is no evidence for universal prototypes for spatial orientation.

Bowerman also looked at how children establish the links between semantics and syntax, i.e. how do they build the formal structure of their grammar from the semantic basis. In this study you have to take into account the fact of the tremendous diversity of language and yet the speed that children acquire language and their ability to home in on language specific constructions. This all suggests innate structures. Yet from the evidence available it is possible to argue either way, that children build syntactic structures from semantic information or, vice versa, that children predict the meaning of a construction from its structure. Bowerman also contends that children make many errors they should not make at all if they are constrained by innate linking rules.

In her last session Bowerman presented the results of experiments to determine if the ontological distinction between objects and what they are made of, their substance, is innate or not. Based on evidence from children learning several different languages, the conclusion was that this distinction is innate.

'Synchronic and diachronic typology of syntactic structures' presented by Ranko Matasović. Ranko Matasović first of all presented a standard overview of syntactic typology beginning with the word order universals proposed by Greenberg (1963). Then he presented the three types of historical syntactic change proposed by Harris and Campbell (1995). REANALYSIS is a mechanism which changes the underlying structure of a

syntactic pattern, and which does not involve any modification of its surface manifestation. EXTENSION is a mechanism which results in the surface manifestation of a pattern, and which does not involve immediate or intrinsic modification of underlying structure. BORROWING is a mechanism of change in which a replication of the syntactic pattern is incorporated into the borrowing language through the influence of a host pattern found in a contact language. An example of the latter outside of the IndoEuropean context is presented in Roberts 1997. The Austronesian languages found in Madang Province of Papua New Guinea have adopted the SOV basic word of the neighbouring Papuan languages. In addition they have developed medial, dependent forms as found in the Papuan languages. One Austronesian language, Dami, has taken the morphosyntactic borrowing a stage further, however. This language has developed a switch-reference system replicating the switch-reference systems found in the surrounding Papuan languages. In comparing Dami with the other Austronesian languages it can be seen that what were historically realis/irrealis markers now indicate same subject following and what were historically simultaneous tense markers now indicate different subject following.

Matasović also introduced the important work of Johanna Nichols who proposed the notion of head-marking and dependent-marking grammar in Nichols 1986 and who also introduced the principle of diachronic stability in Nichols 1992. This principle states that semantically and cognitively syntactic features will be stable over time but pragmatically motivated features will be unstable. So, since word order is pragmatically motivated it changes frequently over time.

Matasović then presented a number of reanalyses of diachronic syntax in IndoEuropean languages. At the end Matasović presented several reasons why RRG is superior to generative models of grammar in terms of diachronic syntax. Generative grammar is critically dependent on syntactic categories, phrase structure, subordination and abstract levels. Consequently it requires a universal notion for clause structure of NP + VP, at some level of abstraction. However, the VP as a constituent does not figure in diachronic changes of syntactic structure. Instead it is the constituents of VP, i.e. verb and direct object, which change position over time. Therefore VP is not a concept that persists over time. So it is neither a synchronic nor a diachronic universal concept in language. In RRG clause structure is based on the notions of predicate and argument(s). RRG has a concept of operators which have scope over different levels of clause or NP structure. In diachronic change it has been established that operators with a broader scope tend to develop into operators with a narrower scope, but not vice versa. For example, tense (clause level operator) and directionals (core level operator) develop into aspect (nucleus level operator), but not vice versa.

'The generative lexicon and semantic theory' presented by James Pustejovsky. For me this course was the most understandable, stimulating and interesting. Pustejovsky 1998 presents a novel theory of lexical semantics that addresses the problem of "multiple word sense", i.e. how we are able to give an infinite number of senses to words with finite means. For example, the verb *enjoy* has these basic senses listed in the Collins COBUILD dictionary: (1) find pleasure or satisfaction in doing something and (2) be lucky to have. Now consider the following uses of *enjoy*. In each case the precise sense of *enjoy* is determined by its complement. So, how Jane enjoyed the movie is different from how Elizabeth enjoyed the route.

Jane enjoyed the movie.
Alexis enjoyed the proof.
Sheila enjoyed the novel.
Elizabeth enjoyed the route.
The goat enjoyed the book.
John enjoyed the book.

How are we able to understand a novel use of *enjoy*, such as *the goat enjoyed the book*? The goat's enjoyment will surely be different to that of John's. If our model of lexical semantics has a static list of senses for each word then it would be difficult to explain how a novel use is understood. As an answer Pustejovsky proposes a generative approach to word meaning. This is a formally elaborated theory of generative lexical semantics. It provides for an implemented computational treatment of word meaning that connects explicitly to a compositional semantics.

The essence of Pustejovsky's theory is that the lexicon functions generatively, first by providing a rich and expressive vocabulary for characterizing lexical information; then by developing a framework for manipulating fine-grained distinctions in word descriptions; and finally, by formalizing a set of mechanisms for specialized composition of aspects of work description such that, as they occur in context, extended and novel senses are generated.

Epilogue. Afterwards Van Valin hailed the whole conference as a great success and hoped to have more of the same in future years. I found it was very helpful to meet Robert Van Valin and some of the other linguists involved in developing RRG. I was also able to learn a lot more about RRG by speaking to the originator himself. At SIL UK we are very interested in switching to RRG as the main model of linguistic enquiry that we teach. In order to pursue this goal we have arranged for Professor Van Valin to come to Horsleys Green in 2001 to run a RRG Workshop from 6–17 August. We are hoping that many SIL field linguists will come to this workshop. If you are interested then please contact me at the the addresses given below.

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Electronic Data Series

For several years, SIL has had a website for Electronic Working Papers, at http://www.sil.org/silewp/. This is for papers which are of interest and should be gotten out to the larger linguistics community, but perhaps were not yet polished enough or too specialized for journal publication.

For some time, we have dreamed of adding an Electronic DATA Series to this. A paper in the EDS would be organized data, with minimal analysis. Examples of this could be basic word lists, verb and noun paradigms, and interlinearized and annotated texts. Besides the written documentation, we hope to eventually include sound files with many of these, so the browser could read the word, click on the icon and actually hear what it sounds like. A bit intimidating to expose your transcription to the world, but I predict this will be more and more the pattern with language-based web sites around the world. We're still at the discussion stage; let me know what you think, and if you might be willing to contribute to this series.

—Michael Cahill
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